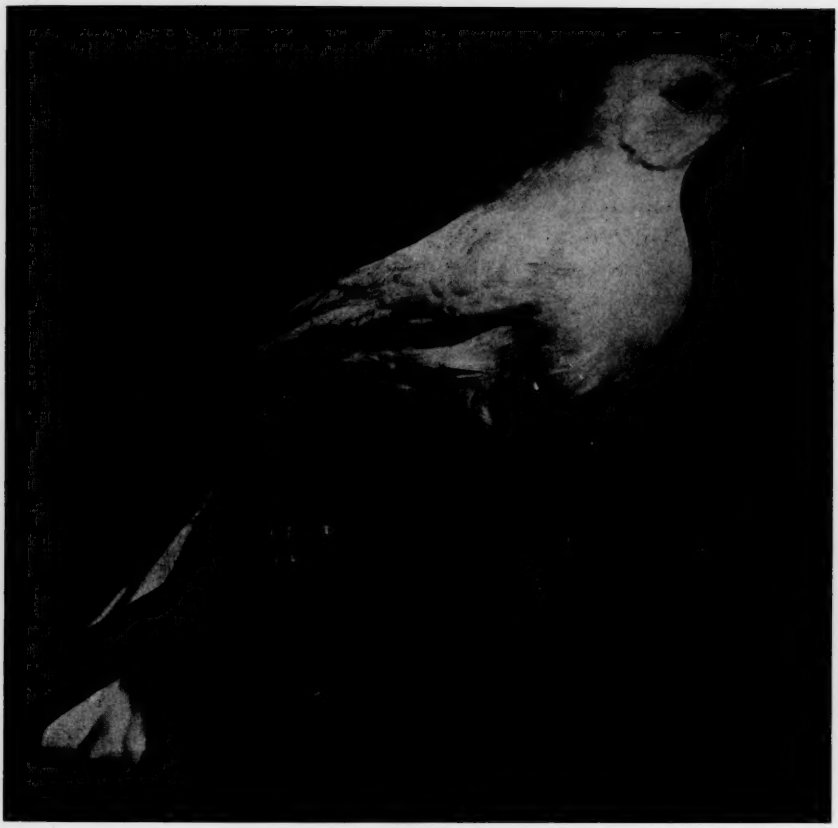
The Journal of HEREDITY



AN ALBINO MOCKINGBIRD

(SEE PAGE 433)

The Kink-tailed Mouse

Keys Quadruplets Test Heredity-Environment

Seed-formation of Kentucky Bluegrass

How to Get Blood From a Rat

VOLUME 31 OCTOBER, 1940 NUMBER 10

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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The JOURNAL OF HEREDITY is published monthly by the AMERICAN GENETIC ASSOCIATION at 32nd Street and Elm Avenue, Baltimore, Md., and Victor Building, Washington, D. C. Address all communications regarding editorial matters to Room 308, Victor Building, Washington, D. C. Communications regarding the business management of The JOURNAL OF HEREDITY may be addressed to the publication office, 32nd Street and Elm Avenue, Baltimore, Md., or to the Victor Building, Washington, D. C. Entered as second-class matter February 24, 1923, at the postoffice at Baltimore, Maryland. Acceptance for mailing at the special rate of postage provided for in section 1103, Act of October 3, 1917, authorized December 20, 1918.

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$3.50 a Year. 35c the Copy. Prices of back numbers on application.

The

Journal of Heredity

VOL. XXXI OCTOBER, 1940 No. 10

CONTENTS

PHYSICAL AND MENTAL TRAITS OF THE COLLEGE QUADRUPLETS
By Iva C. Gardner and H. H. Newman 419
INHERITANCE OF A TAIL ABNORMALITY IN THE HOUSE MOUSE
By Ernst Caspari and Paul R. David 427
ALBINISM IN MOCKINGBIRDS
By E. A. McIlhenny 433
ACHONDROPLASIA IS ALWAYS HEREDITARY AND IS INHERITED DOMINANTLY
By Ernst Trier Mörch 439
METHODS OF SECURING BLOOD FROM RATS
By S. O. Burhoe 445
COLD STORAGE COLTS
By V. R. Berliner, F. E. Cowart and L. L. Pharis 449
INHERITANCE AND GROWTH BEHAVIOR IN A SECTORIAL CHIMERA IN THE CANNA
By Charles Dallas Blachly 453
PROGENY TEST AS A MEASURE OF TYPES OF SEED-DEVELOPMENT IN POA PRATENSIS L.
By Fred W. Tinney and O. S. Aamodt 457

Notes and Reviews

CENSUS OF IDEAS REGARDING DROSOPHILA (Rev of MULLER: *A Bibliography of Drosophila*) 424
WHY WE ARE FAT OR VICE VERSA (Rev. of RONY, *Obesity and Leanness*)
By W. Landauer 426
A CRITICAL NOTE ON DIFFERENTIAL CLASS FECUNDITY IN BRAZIL
By Barbara S. Burks 431
THE BOTANIST'S MICROSCOPE (Rev. of JOHANSEN, *Plant Microtechnique*) 451
ARE THEY IDENTICALS? 454
PATHOLOGICAL HEREDITY (Rev. of LANGE, LENZ, VON VERSCHUER, and WEITZ, *Erbpathologie*)
By Morton D. Schweitzer 464

STUDENT SUBSCRIPTIONS

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FOUR COLLEGE GRADUATES

Frontispiece

The Keys quadruplets are the only living quadruplets who have all graduated from college, and probably the only quadruplets who have graduated as a group. In accomplishing this unique feat they have as a by-product made a valuable contribution to our knowledge of heredity and environment. Here we have a chance to measure the effects of a similar environment on a pair of identical twins, and simultaneously on a pair of fraternal twins. Roberta and Mona, the identical twins still can't be told apart. They have made remarkably similar scholastic records. Mary and Leota, the fraternal twins, differ from each other and from their identical sisters in many characteristics. In personality tests and in their scholastic records they differ about as much as they do physically, showing that a similar environment has not molded these individuals into the same pattern. Standing: Mona, Mary, Roberta; seated: Leota.

PHYSICAL AND MENTAL TRAITS OF THE COLLEGE QUADRUPLETS

"Double-Twins" Test Nature-Nurture Problem by Exposing Paired Identical and Non-Identical Heredity-Complexes to a Similar Environment

IVA C. GARDNER, *Baylor University*
and
H. H. NEWMAN, *University of Chicago**

THE Keys quadruplets, Roberta, Mona, Mary and Leota, are probably the best known quadruplets in America. Their photographs have often appeared in the pictorial sections of our newspapers throughout the land. They have appeared in public on many occasions, chiefly as good-will ambassadors for the Oklahoma Legislature. They have lived together until very recently when one of them was married.

As we shall attempt to show in ensuing paragraphs, these quadruplet girls are really two pairs of twins, Roberta and Mona being identical (one-egg) twins and Mary and Leota fraternal (two-egg) twins. Thus we have in this set the coincidence at one birth of the two modes of twinning. It is rather remarkable that they should all be of the same sex, for there is only one chance in eight of three-egg quadruplets being all girls.

We have designated these attractive girls "College Quadruplets," for it seems certain that they are the only *living* set, perhaps the *only* set of quadruplets, living or dead, who have ever graduated as a complete group from college. They were born in Hollis, Oklahoma, on June 4, 1915. They entered Baylor University, Waco, Texas, as freshmen in the autumn of 1933, and all received the A.B. degree in June 1937. All of their college grades are available for comparison, but these grades are not so significant as are their performances on standard intelligence tests, for they did not all elect the same courses of study.

The points of particular interest in the present study are two: (a) although they have been tentatively diagnosed as a three-egg set, no careful, expert diagnosis has previously been made by one experienced in twin diagnosis; (b) they were previously studied at the age of twelve by Shirley L. Brintle, who published her results in the *Journal of Genetic Psychology*, Vol. 39.

It seems well worth while, therefore, to offer a diagnosis based on well-tried methods and to repeat the study of mental traits, after a period of thirteen years, to determine whether or not their relative mental ranking has changed or has remained constant.

Zygotic Diagnosis

As may be seen in the photograph (Figure 1) Mona and Roberta, the two at the right, are extremely similar. They are constantly confused by their friends. It is a commonplace for Mona to have to say to her friends, "I am not Roberta. I am Mona." They not only look alike in a general way, but correspond unusually closely in all their physical features, as will be evident from a perusal of the table giving their physical characteristics (Table I). Leota differs in almost every way not only from the "twins," Roberta and Mona, but in a multitude of ways from Mary. The only possible question that could arise as to the zygosity of these quadruplets is whether Mary might not have a closer-than-sibling relationship to Roberta and Mona. A glance at Table I shows that

*As in two previous papers published in this *Journal* by these two collaborators, Dr. Gardner gathered the data and Dr. Newman on the basis of these data, has made the diagnosis of zygosity and has written up the case for publication with Dr. Gardner's concurrence.—ED.

Mary differs from the "twins" in many respects in which the "twins" are concordant. Mary's iris color is distinctly darker brown, her ear lobe is attached (that of the "twins" unattached), she has freckles (of which the "twins" have none), she is more robustly built and all her bodily circumferential measurements are well in excess of those of the "twins." She is nearly two inches shorter than the "twins," but weighs nearly 30 pounds more than either of them. These differences should in themselves be sufficient to assure us that Mary is merely a sibling of the "twins," Roberta and Mona, but a study of palm and finger prints more than redoubles our confidence in this conclusion.

Dermatoglyphics

These prints were made by the Police Department of Waco, Texas, under the direction of Dr. E. N. Jones, Dean of Baylor University. Dr. Jones also superintended the taking of physical measurements.

Finger Patterns

	Left hand	*Right hand*
Roberta:	U R^a U U U	U A U U U
Mona:	U A U U U	U U^a U U U
Mary:	U R R U U	U R U U U
Leota:	W^a R R U U	U U U U U

Out of 40 finger patterns there is only one whorl (the thumb of Leota's right hand), 31 of the patterns being ulnar loops. Mary has three radial loops, Leota two and Roberta a minimal radial pattern hardly more than an arch. Roberta and Mona are alike in having a very low ridge count in digits 2 and 3 of both hands. There is nothing of great diagnostic value in these pattern formulae, but when we come to quantitative values (ridge counts) of these patterns, the diagnosis becomes conclusive.

Ridge Counts

The total number of ridges is given for the two hands separately.

	Left hand	*Right hand*	*Total*
Roberta	32	42	74
Mona	25	37	62
Mary	64	73	137
Leota	98+	100+	198+

Roberta and Mona show a total difference of only 12 ridges, which is only a little above the average for identical twins. In both individuals the thumb and fourth finger in both hands have moderately high ridge-count values, but the other fingers have greatly reduced ridge counts. Mary shows a slight tendency to low ridge counts on digits 2 and 3, but all the other patterns are rather high in ridge count. The total of ridges in Mary exceeds that of Mona by 75 ridges, and Roberta by 63, differences nearly twice as great as those found between any of our identical twins. The patterns of Leota are extremely large with high ridge counts on all fingers. Both patterns on digits 5 are so large that the prints do not show the triradius. It is probable that the total ridge count exceeds 200. Hence Leota's total ridge count exceeds that of Mary by at least 61 ridges. To those familiar with ridge counts in twins these data alone would prove beyond question that Leota and Mary have no closer than sibling relationship to the "twins," Roberta and Mona. This is one case where ridge counts alone would have been sufficient to diagnose this set as trizygotic.

Palm Patterns

The right hands of Roberta and Mona are identical in formulae and almost so in detail. Each of them has the rare radial arch pattern in the hypothenar region. The two left palms are, however, not strikingly similar. Homolateral resemblances are far greater than bilateral, which is a good criterion of monozygosity for the pair, Roberta and Mona. None of the other pairings (Roberta-Mary, Roberta - Leota, Mona - Mary, Mona - Leota, Mary - Leota) show even

Palm Formulae

	Left hands	*Right hands*
Roberta	7.5″ .5′ .h-t -L^r .O.O.O.L	9(8). 7(6).5′.3 -?-A^r.O.O.O.L
Mona	9.X .5′ .3-?-A^u.O.O.O.V	9(8). 7(6).5′.3 -?-A^r.O.O.O.L
Mary	7.5″ .5′ .3-t-A^u.O.O.O.L	7(8).5″(6).5′.4 -t -A^u.O.O.O.L
Leota	9.0 .5″ .3-?-L^r .O.O.O.V	9 .0 .5″.5′-t-L^r.O.O.O.M

as great homolateral as bilateral resemblances, which tends to prove that only sibling relationship exists between the members of these pairs. The dermatoglyphic method of diagnosis agrees fully with that based upon general similarities, and is in our opinion conclusive.

Intelligence Tests

Miss Brintle's Studies of the Quadruplets at 12 Years of Age

At 12 years of age three intelligence tests were given by Miss Brintle to the Keys quadruplets with the following results:

I. Q's on Three Tests

	Binet	Otis	National
Roberta	112	128	123
Mona	108	129	117
Mary	98	104	99
Leota	110	124	119

At that time the differences between Roberta and Mona were less than the average for 50 pairs of identical twins reared together. Leota is also less different from Roberta and Leota than the average for identical twin pairs, but Mary consistently ranks many points below the other three on all tests.

Much the same situation was found for the Stanford Achievement test, which showed the following mental ages: Roberta, 15.8; Mona, 15.1; Mary, 13.8; and Leota, 15.2. Grade School grades and averages showed the same ranking, but with less striking differences. Possibly there was an unconscious effort to maintain an equality. The High School averages, however, were less similar:

High School Averages

Roberta	92
Mona	90
Mary	83.7
Leota	87.7

Thus Roberta and Mona, the "twins," were the highest and the most similar, while Leota had dropped behind. Mary still was the lowest.

The Baylor University Studies

The first test given was the American Council on Education Test, on which the scores and percentile ranking were as follows:

	Score	*Percentile*
Roberta	198	81
Mona	182	69
Mary	141	45
Leota	157	49

Roberta, who from the earliest obtainable data has always ranked highest in the group, maintained her lead. Mona dropped her status somewhat, Leota had lagged behind either of the "twins," while Mary was distinctly the lowest.

The honor-point averages for the four college years were as follows: Roberta, 4 8/9; Mona, 4 1/6; Mary, 3 7/12; and Leota, 3 11/18. It is interesting to note that scholastic performance, in spite of the fact that there was some variation in scholastic programs among the four girls, could have been predicted almost exactly on the basis of the scholastic aptitude test given when they entered college. The ranking remains as it was in High School: Roberta first, Mona second, Leota third, and Mary fourth.

Dr. Gardner thinks that the drop of Leota from almost equality with the "twins" to a much lower scholastic level may have been due to the fact that she majored in speech and was much more interested in extra-curricular activities than were the other three. She spent much time on dramatics and was chosen in her senior year as the most representative girl of Baylor.

Personality Traits

The four sisters always dressed alike and, when together, their responses were quadruplet group responses rather than individual responses. When apart, however, they showed marked individual differences. Roberta and Mona were seldom separated and always tended to act as a unit, but the other two showed distinct personality differences. Leota seemed to be the most independent of the group, spending most of her time with other students. Mary was always the jolly, jovial and witty one, and seemed happier than the others. The "twins" were extremely similar in behavior and reactions and could be dis-

THREE STILL LOOK ALIKE

Figure 1

The Keys quadruplets have always been in the public eye. They broke into the JOURNAL OF HEREDITY in May, 1916. At that time Roberta, Mona and Mary were supposed to be identical, and Leota was spotted as a "mere sister." In the JOURNAL OF HEREDITY for October, 1916, it was suggested that Leota's different eye color (hair color being more alike at that time) was due to a somatic mutation, the four girls being really identical quadruplets. Time has proved the error of this suggestion. Today Leota differs in many characteristics other than eye color, and Mary weighs considerably more than her identical twin sisters and is two inches shorter. Left to right: Mary, Leota, Roberta, Mona.

tinguished by but few of their acquaintances.

Several personality tests were given, of which we shall record but two, Bell's Adjustment Inventory and Bernreuter's Inventory. (See below.)

On the Bell Adjustment Inventory Test, the total score of each of the Quads, when compared with the norms of high school and college students, fell in the group that was designated as "good." When the records of all the other senior girls were stacked at random in piles of four each, not a stack contained four adjustment scores designated as "good," when compared with the same norms. Of these other four-girl groups, the one coming closest to the record of the Quads in adjustment rating, according to the records of this test, showed thirty maladjustment points more than the quads. Of the four Quads, Mary showed the fewest points of maladjustment, with Leota second; while Mona and Roberta, the identical twins of the group, with only a single point's difference between them, showed eleven and twelve points respectively more than Leota.

The Bernreuter Personality Inventory shows Roberta and Mona to be much more similar than any other pair in characteristics measured by the test. Being quads and subjected to the same external environment, therefore, does not make the girls more alike in personality unless there is identical heredity, as is the case for Roberta and Mona.

Leota and Mary proved to be more dominant than the twins on the test. The dominant scores of Roberta and Mona are more like each others' scores than when compared separately with the other sisters.

The quads show a better average on this test on all measures than the average of the fifty other senior girls. The Keys Quadruplets are as well adjusted

Table I. Physical Characteristics of The Keys Quadruplets

Measurements made at Baylor University under Supervision of H. H. Jones (With cooperation of Staff of Women's Hospital) May 29, 1937

Characteristic	♀ Roberta		Mona		Mary		Leota	
	As of 9/28/36	As of 5/29/37	As of 9/28/36	As of 5/29/37	As of 9/28/36	As of 5/29/37	As of 9/28/36	As of 5/29/37
Height	67¾"	67 5/8"	66¾"	67"	65¾"	66"	65 1/8"	65 5/8"
Weight	129 lb.	131¾ lb.	131 lb.	131¾ lb.	157¾ lb.	160¼ lb.	146 lb.	149 lb.
Hair color	dark brown		dark brown		dark brown		very light brown	
Hair form	straight		straight		straight		very wavy	
Iris color	medium brown		medium brown		dark brown		very light hazel to blue	
Ear (Lobe attach.	slope to head		slope to head		medium slope		slope to head	
Position (Lobe attach.	unattached		unattached		attached		attached	
Skin color and texture	medium dark tans brown		medium dark tans brown		medium dark tanning brown		light, tans dark brown	little but burns
Handedness	right		right		right		right	
Hair whorl	clockwise (faint)		clockwise (faint)		?		clockwise--sug. of double crown	
Dentition	uniform (sim. to Mona)		uniform (sim. to Roberta)		normal		teeth spaced	
Use of eye in sighting	right		possibly left (not certain)		left		right	
Eye defects	very slight nearsightedness		normal		normal		normal	
Waist	27"		27¾"		30¾"		30"	
Chest (Expanded)	33½"		34"		36"		34"	
								32 7/8"
Pelvis girth	33½"		34½"*		36¾"		33½"	
Femur length (left)	22"		21¾"		21¾"		21"	
Tibula length (left)	15"		15"		14¾"		14¾"	
Calf circumference (left)	12 15/16"		13¾"		13¾"		13¾"	
Foot (right	9 15/16"		9¾"		9 11/16"		9 7/8"	
length (left	9 15/16"		9¾"		9¾"		9¾"	
Foot (right	3 5/8"		3¾"		3 5/8"		3 5/16"	
width (left	3 5/8"	3 5/16"		3 5/8"		3 5/8"		3 11/16"
Hand width (right	3 5/8"	3 11/16"		3¾"		3¾"		3¾"
palm (left	3 7/8"	3 15/16"		4"		4"		3 7/8"
Greatest (right	7¾"	7¾"		8"		8"		7 7/8"
hand width (left	7¾"	7¾"		8"		8"		8"
Olecranon to (R	14¾"	14¾"		15"		15"		14¾"
Finger tip (L	14¾"	14¾"		15"		15"		14¾"

*Right hip protrudes more than left. May be due to auto accident. Back injured--age 17.

Bell's Adjustment Inventory

	Roberta Score	*Roberta* Classification	*Mona* Score	*Mona* Classification
Home	2	Excellent	1	Excellent
Health	3	Good	5	Average
Social	18	Average	13	Average
Emotional	7	Good	10	Average
Total	30	Good	29	Good

	Mary Score	*Mary* Classification	*Leota* Score	*Leota* Classification
Home	0	Excellent	6	Average
Health	1	Excellent	2	Good
Social	2	Very aggressive	2	Very aggressive
Emotional	4	Good	8	Good
Total	7	Excellent	18	Good

Bernreuter Inventory

	B1-N	B2-S	B3-L	B4-D	F1-C	F2-S
Roberta	67%	56%	66%	27%	66%	58%
Mona	67%	24%	73%	26%	72%	58%
Mary	13%	4%	9%	73%	12%	3%
Leota	38%	13%	29%	60%	40%	31%

as any four girls one could find anywhere. In spite of being quads, and in spite of excessive publicity, they are as normal girls as one could find—happy and cheerful. In college they were always ready to cooperate with the student body or faculty in any enterprise. They were very much loved on the Baylor Campus.

Summary

This set of quadruplets, all girls, but derived from three zygotes, offers a very interesting study of the relative effects of hereditary resemblances and differences operating in a common environment. The pair of one-egg twins, Roberta and Mona, have remained through the years very highly similar in every way. They have not tended to grow more like the other sisters in any respect in spite of continuous intimate association with them.

After many years of constant association and with equal educational opportunities, the "twins," Roberta and Mona, have ranked distinctly higher in mental ability and in scholastic achievement than the other two. Mary has constantly been lowest in these respects and Leota intermediate between the "twins" and Mary. There has also been no tendency for these four girls to grow more similar physically or in personality traits.

This study lends strong support to the view that, when several individuals of sibling relationship are reared together in a common environment, their hereditary differences persist and they do not tend to grow more similar as the result of long continued association or long exposure to a common environment. Even the strong group unity characteristic of quadruplets and their tendency to respond to and to be responded to by their associates as a "quad" group have not made them more similar than they would have been had they been a group of sibling sisters.

A CENSUS OF IDEAS REGARDING DROSOPHILA

THE reviewer has on occasion described himself as a midwife of brain-children born into that hypothetical land, "genetica." Such midwifery incorporeal in this esoteric field of endeavor, has complexities never dreamed of by Saranos, Greek obstetrician of the beginning of our era—nor for that matter by the legendary Dr. Allan Roy Dafoe, of Callander, Ontario. The vital statistics of any portion of our genetic terra-still-too-little-cognita has an interest to every student of heredity.

A list of blessed events in the field of Drosophila genetics has recently come from the card catalog of Dr. H. J. Muller, and under the auspices of the Imperial Bureau of Animal Breeding and Genetics.* In this are listed 2,965 intellectual infants of varied ideational paternity who have seen the light of day since the humble vinegar fly began its march to the genetic throne from Morningside Heights, in Hendrick Hudson's metamorphosed New Amsterdam, some thirty years ago. Some population experts are adept in seeing a logistic curve as the graphic representation of the growth of any population. Possibly such a curve also represents the growth of a population of discoveries and of the ideas on which they are based. At any rate, Dr. Muller points out that the number of citations stood at 381 in the bibliography included in *The Genetics of Drosophila*, published in 1924. That mid-point of the nostalgic twenties represented about the first half of the Drosophila era. The second half of the fruit-fly age yielded 2,584 titles or about eight times as many

*Muller, Herman Joseph. A Bibliography on the Genetics of Drosophila. Pp. 132. $1.25 (5 shillings). Edinburgh, Oliver & Boyd, 1939.

as the first half. Evidently our population of intellectual brain-children is growing at a rate, which, if the principles of vital statistics apply in this intangible field, presages in the world of genetic ideas a Malthusian day of reckoning with growing epidemics of idiational cholera-morbus inevitably to be expected.

Ideas grow, fortunately, on an ever-normal granary of intangibles and a quick survey of what goes on in Drosophila genetics today does not suggest that there exists any immediate danger of over-population of ideas. This has not always been so. Morgan, in his Presidential address before the 1932 International Genetics Congress, had envisioned no Great New Continents, which could be the home of new teeming millions of genetic brain-children. It looked as though the world of Drosophila genetics was beginning to fill up—as though "standing room only" were soon to be the order of the day. Only two years later, Heitz, Bauer, Painter and others, discovered that in Drosophila such a continent had existed under the very noses of genetic researchers, and that it had been sighted, and roughly charted half a century before. This new land—this land of bands and lucent areas—consisted of the giant salivary gland chromosomes of the amazingly ingenious Drosophila, which always, like the widow's cruse, yields abundantly to the seeker, even after many years. The salivary gland chromosomes, huge as they relatively are in comparison with the garden variety of chromosome, are still small, nay tiny, objects. Nevertheless they have proved to be big enough so that hundreds of Drosophila researchers are today lost in a jungle of bands and clear areas of nucleic acid, of position-effect, and of a multitude of other wonderful and awful micro- and sub-micro-physiographical features.

Research Plans

When we are tempted to talk solemnly about the "planning" of research consider the history of Drosophila. Morgan and his students chose Drosophila in the begining because the flies could be easily raised in the laboratory, had a short life cycle (so that several generations could be raised a year), and a conveniently small number of chromosomes. Out of this galaxy of genetic virtues and the essential galaxy of keen minds, grew the gene theory. Then in 1924-25, knowledge of the fruit fly had grown detailed enough so that Muller was able to set up his x-ray experiments in such a way that the induction of mutations by x-rays could be demonstrated. Since then, induced mutations have been found in a great number of other organisms. Few other organisms have been found which lend themselves so conveniently to radiation technique as Drosophila. To top this series of lucky breaks—which the smartest planning-board could not have foreseen—came the salivary gland chromosomes.

All of the developments (up to 1928) had enabled geneticists and physiologists to push our knowledge of heredity very far into the unknown, and even to prove cytologically such genetically demonstrated phenomena as translocation. Things are arranged in the normal cell on such a tiny scale that no direct study was possible of the effects obtained by eliminating or moving around pieces of chromosomes. Utilizing chance breaks and shooting chromosomes with x-rays had made possible a genetic attack on this problem of chromosome architecture. Much had been done, but more remained and the method of statistical inference was beginning to reach something of a dead end. Just at this juncture Painter showed that the giant salivary gland chromosomes were prime cytological material for pushing on further into the still mysterious regions of the physical location of those strange micro-organs, or chemical laboratories (call them genes or what you will), which somehow control the development of living organisms. Some of the most fundamental propositions of genetics had been pure inference before the genetic virtues of these super-chromosomes were discovered. Now it was possible to put on a solid observational basis several foundation-stones of that imposing edifice

of statistical deduction and ingenious inference which the theory of the gene had become by 1934.

Try to plan that amazing chain of lucky coincidences in advance! And try to predict what will happen next!

All of this but goes to show the kind of exciting things which have gone on to produce those primly ranked brain-children on parade in Dr. Muller's bibliography. Those who have attempted to keep track of all the varied circuses which have been staging a continuous performance in this astonishing field of Drosophila genetics will encounter many other equally complicated and engaging trains of thought. Since the bibliography is not annotated in any way, the cues for flights of recollection and speculation will have to come from inside the reader's head, and the elect will find these rather forbidding pages fascinating. The bibliography will, of course, have much more important uses than to serve as a springboard for such day dreams.

Its author holds such high place in his chosen field that it would be presumptious for an outsider to even look for technical criticisms. There is, however, one aspect of the matter which is unfortunate from the editorial point of view. The references in the bibliography are not numbered as is sometimes conveniently done. Such a numbered bibliography could help with a problem growingly confronting editors, what with the field of genetic knowledge ever broadening, and with the demand for publication space becoming more acute. Were all bibliographies serially numbered as was the U. S. Department of Agriculture's *bibliography of plant genetics and plant breeding,* (1934), it would be possible to get many more references into less space than with the conventional form of citation. Were the references in Muller's bibliography numbered, a dozen authors might be cited in eight or ten lines, instead of in half a page, by simply referring to M106, instead of to: Doe, John. Cuprozinc Bands in the Salivary Gland Chromosomes of Obscurifolia. *Journal of Cytogenealogy* 30:170-198. 1935. And the burden of "literature cited" grows apace as science pushes forward into the unknown, making such bibliographic shorthand almost a necessity.

Questionably all the Drosophila laboratories of the world should add this modest but invaluable publication to their libraries. All other biological laboratories and all libraries attempting to cover the literature of science will also find it essential. It is a most valuable addition to the literature of science, and one which involves an outlay of work out of all proportion to the size of the published result.—R. C.

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Why We Are Fat—or Vice Versa

GENETICISTS know from studies with various animals that "normal" body weight is a complex trait, produced by the interaction of many hereditary factors and of a multitude of environmental conditions. Aberrations from typical body weight, obesity and leanness, are of great intrinsic interest, but, in addition, may offer the most promising material for gaining an insight into the mechanisms which are at work in the determination of normal body weight. From this point of view the book* by Rony should be of great interest to geneticists. Although the author is primarily interested in human obesity and leanness, much of the information contained in this monograph should be applicable to other forms. The various chapters deal with metabolism and transport of fat, the fat deposits, the caloric balance, endocrine factors, the nervous system, intermediary metabolism, lipophilia, heredity, classification and diagnosis, clinical pathology and therapy of obesity and leanness.

W. Landauer

Storrs, Conn.

*Rony, H. R. *Obesity and Leanness.* 300 p., 32 figures. $3.75. Philadelphia, Lea and Febiger, 1940.

THE INHERITANCE OF A TAIL ABNORMALITY IN THE HOUSE MOUSE

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SEVERAL mice exhibiting what appears to be a new mutation were observed in 1937 in the stocks of Mr. S. P. Holman of Manatee, Florida, and were sent by him to Professor L. C. Dunn of Columbia University. The aberrant mice had kinky tails resembling the mutation "Fused" described by Reed,† and showed as well other characteristics (waltzing, deafness) which will not be discussed here. Three inbred kink-tailed lines were produced from outcrosses of the Holman stock to three stocks of the Columbia colony, namely Little's dilute brown, the Bagg albino, and a blue spotted line, "Columbia K." Mice from these three strains (designated respectively as stocks I, II and III) were obtained from the Columbia laboratory and have been inbred by us for seven or more generations. Subsequently, animals from the inbred strains have been outcrossed to a normal albino stock which was originally secured from Dr. E. C. MacDowell, and which has been inbred by brother × sister matings in this laboratory since 1934. This stock will be hereafter referred to as the "Lafayette albino."

Description of the Abnormalities

The characteristic expression of the Kink character is a tail flexure which may vary in degree from a barely perceptible bend or bump to a strong spiral twist. In addition, especially in the more extreme Kinks, the tail may be reduced to as little as one-half of its normal length (Figure 2). On the other hand, animals have appeared which by external inspection could not be recognized as Kinks, and whose Kink genotype could be revealed by breeding experiments or by clearing. Bifurcations of the tail occur very rarely.

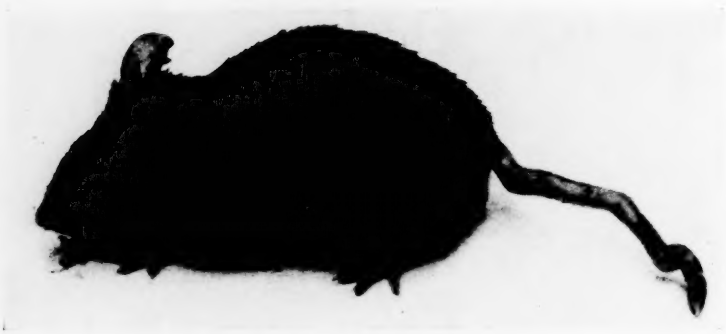
The skeletons of Kink mice have been studied in X-ray photographs (Figure 3) and in specimens cleared in potassium hydroxide and stained with alizarine red.

The number of vertebrae in kinky tails is more or less reduced as compared with normal. Some vertebrae show abnormalities in structure. One side may be shorter than the other one, causing the next following vertebra to form an angle with it. Relatively often more or less round or triangular bones are found in Kink tails. These bones which may be rudimentary vertebrae are always situated eccentrically. They may be free or fused with one or both of the adjacent vertebrae. Because of their eccentric situation, the more distal vertebra is often inserted at an angle to the more proximal one, resulting in the externally visible kinks. This can be well observed in the three last vertebrae of Figure 3*A*. Angulation may occur in a dorsoventral, in a frontal, or in any other plane. If several eccentric bones situated in different planes follow each other closely, a twist in the tail results. Furthermore, two neighboring vertebrae may be fused, thus producing stiff segments, as may be seen in several vertebrae near the base of the tail in Figure 3*A*.

Abnormalities have also been found in sacral, lumbar and thoracic vertebrae of Kink animals. They consist of ankyloses between adjacent vertebrae and of eccentric reductions of single vertebrae (Figure 3*C*, fourth lumbar vertebra). These reductions go as far as the complete disappearance of one half of a vertebra. If this happens in thoracic vertebrae, the corresponding ribs may become affected, fusing partly or completely to the adjacent ribs. Partial and total fusions be-

*Fellow of the John and Mary R. Markle Foundation.

†REED, S. C. The inheritance and expression of Fused, a new mutation in the house mouse. *Genetics* 22:1-13. 1937.



MOUSE WITH A STRONG TAIL-KINK

Figure 2

The kink gene affects the formation of the vertebrae. Depending on how many vertebrae are affected, the abnormality of the tail may vary from a deviation not visible by external observation to a corkscrew twist such as is shown here. There is strong evidence that the kink gene is lethal when it is inherited from both parents.

tween up to five ribs have been observed. The variations observed are in general similar to those found by Reed in "Fused" mice.

Breeding Behavior

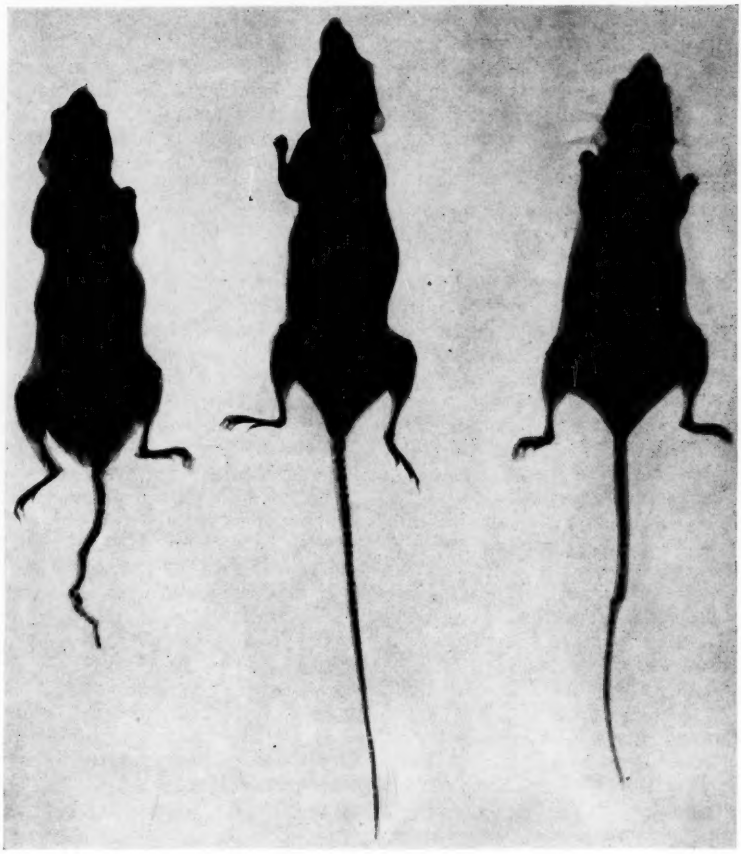
The breeding data presented here are based on the diagnosis of the Kink character on the day of birth. The descriptions were checked in the full-grown mice except for those individuals which failed to survive. Comparison of descriptions of 293 mice from undepleted litters at birth and at weaning showed that in 23 cases the description made at these two occasions differed. In 12 cases an animal which was diagnosed "normal" at birth was described as "Kink" at weaning. In 11 cases the difference was in the opposite direction. As a matter of fact, the breeding results lead to the same conclusions whether descriptions at birth or at weaning are used.

All cases where diagnosis was not certain have been excluded: These uncertain animals were either partly eaten by the mothers, or their classification at birth was not clear and could not be checked at weaning.

The results of crosses involving Kink are shown in Table I. It is obvious from the outcrosses to the normal Lafayette strain that the condition depends on one dominant gene. Heterozygous animals which were obtained by outcrossing Kink animals from the inbred lines to the normal stock, were backcrossed to the Lafayette strain. They gave proportions of Kink progeny reasonably close to the expected 1:1 ratio, namely $47.4 \pm 2.6\%$ and $47.9 \pm 3.2\%$ Kink in the reciprocal crosses. (Table I, 2 and 3.)

The percentage of Kink progeny in F_2, obtained by *inter-se* matings of heterozygous Kink animals from outcrosses to the Lafayette strain, is however significantly different from the expected 3:1 ratio (Table I, 4). As it is not significantly different from a 2:1 ratio, the assumption of a lethal action of the gene for Kink in homozygous condition would explain the ratio obtained. Several observations tend to support this hypothesis:

I. If no lethal action were assumed, true breeding kinky strains should appear after several generations of inbreeding, or there should be at least a higher percentage of Kink among the progeny of Kink $\times$ Kink mice within the inbred strains than in the F_2 from outcrosses, as some homozygotes should appear in the inbred strains. Actually, brother $\times$ sister and parent $\times$ offspring matings



VARIATIONS IN EXPRESSION OF KINK GENE

Figure 3

A—X-ray photograph of a mouse with a strongly kinked tail. *B*—A normal mouse. *C*—A mouse with slight expression of the kink gene. In this instance the abnormalities were mostly of the lumbar vertebrae and could not be detected by external examination. Such a mouse may produce progeny having the extreme expression of the kink gene.

for seven and more generations failed to establish true-breeding strains. The crosses have, in the aggregate, yielded 61.0±1.6% Kink-tailed mice in a total of 942 animals (Table I, 5, 6 and 7 together). This ratio is not distinguishable from that obtained in the F_2 from the outcrosses.

Similarly, when Kink mice from the inbred strains were outcrossed to our stock of inbred albinos, the F_1 contained 48.6±4.9% Kink (n=105, Table I, 1). This result is almost identical with the 47.6% Kink found in the backcrosses of F_1 to the Lafayette strain.

The similarity of the percentage of Kink in F_2 and in the offspring from Kink × Kink matings in the inbred strains, as well as the almost identical ratios found in F_1 and in the backcrosses of F_1 animals to the normal stock, strongly suggest that the Kink animals from

our inbred strains which were selected at random for the crosses were heterozygotes. It seems therefore that homozygotes at least were not present in our material in theoretically expected numbers.

II. Likewise, if Kink were not lethal in homozygous condition, inbreeding should increase the proportion of homozygotes. Consequently, there should be an increased percentage of Kink progeny from Kink × Kink crosses in the later generations. Our data, however, show no increase: Kink × Kink of inbred generations 3-5 inclusive produced 60.8± 2.1% Kink progeny; those from generation 6-8 gave 59.7±2.7% Kinks.

III. Furthermore, the same conclusion is reached if kink-tailed offspring from Kink × Kink crosses are progeny-tested by mating them either to kink-tailed sibs or to normals from the Lafayette strain. Of 91 animals tested in this way, 84 segregated, indicating that they were heterozygotes. The remaining seven animals gave Kink progeny only, but this cannot be taken as evidence that they were homozygotes, since each of them yielded only few (3-8) offspring.

IV. Finally, if the assumption of lethal action of the Kink gene in homozygous condition is correct, the litter size produced by Kink × Kink crosses should be about 25% smaller than litter sizes in comparable crosses of Kink × normal. Ninety-four matings of Kink F_1 animals by normals from the Lafayette strain yielded an average litter size of 6.79±.17 young. Twenty-five matings of Kink F_1 mice by Kink sibs, on the other hand, gave an average litter size of 5.44±.43 young. The decrease of litter size in F_2 as compared with the backcrosses is 19.9% and is 2.93 times its standard error.

All these observations lend support to the hypothesis that the number of homozygotes must be at least considerably diminished as compared with expectancy. On the other hand, actual homozygotes have not yet been found. Experiments to test more directly the lethal action of the gene for Kink in homozygous condition are in progress.

It is noteworthy that in all kinds of crosses, even under the assumption of complete lethality of the homozygotes, a slight but consistent excess of normals is observed. This excess reaches the level of significance only in the relatively large material from inbred stock I. Whether this is due to a somewhat increased lethality of the heterozygotes or to the appearance of phenotypically normal, but genotypically Kink animals, cannot be decided. It has, however, been mentioned already, that faulty classification of genotypically Kink animals indeed occurs.

The occurrence of these apparently normal animals has been demonstrated by mating phenotypically normal animals from sibships containing Kink mice to Lafayette albinos. Of 41 animals tested, 16 proved to be genotypically normal, giving more than 20 normal and no Kink progeny each. Twenty more were probably also genotypically normal, giving from 10 to 19 normal and no Kink progeny. Five were genotypically Kink, producing 11 Kinks among 37 offspring. One of these animals was a waltzer: the remaining four could not be distinguished from normal animals by external inspection.

The Kink factor is strikingly similar

TABLE I.—Breeding Results

Kind of crosses	Total	Kink	Normal	% Kink±S.E.
(1) ♀Kink (inbred)×♂Lafayette normal	105	51	54	48.6±4.9
(2) ♀F_1 Kink×♂Lafayette normal	386	183	203	47.4±2.6
(3) ♀Lafayette normal ×♂F_1 Kink	238	114	124	47.9±3.2
(4) F_1 Kink×F_1 Kink	132	81	51	61.4±4.2
(5) Kink×Kink (I)	687	410	277	59.7±1.9
(6) Kink×Kink (II)	199	128	71	64.3±3.4
(7) Kink×Kink (III)	56	36	20	64.3±6.4

in phenotypic expression to the "Fused" mutation described by Reed. There are also similarities in genetic behavior, both factors behaving as dominants. But in other respects they seem to differ in their genetic behavior. Fused is characterized by striking differences in the percentage of Fused progeny from reciprocal outcrosses to normal strains. In backcrosses of heterozygous Kink to normal animals, however, no difference between reciprocal crosses could be found. (Table I, 2 and 3). Furthermore, homozygous Fused animals seem to be viable, whereas indication exists that Kink is lethal in homozygous condition. This difference in genetic behavior could be explained either by the assumption that different genes are involved, or by the hypothesis that different modifiers are present in the two sets of material. Since both Reed's and our material are derived from mice which have been outcrossed to several diverse normal strains, and both sets of material exhibit reasonable internal consistency for the results of similar crosses, the first assumption, that the two characters are dependent on different genes, seems more likely.

Summary

1. A strain of mice is described, characterized by vertebral abnormalities which result in kinky tails and abnormalities in backbone and ribs. Deafness and waltzing are found in the same strain.

3. Kink tail is inherited as an autosomal dominant, with high or complete lethality in homozygous condition.

3. The Kink gene is probably not identical with Fused, to which it is similar in its expression.

Acknowledgments

The authors wish to express thanks to the John and Mary R. Markle Foundation for financial support of this investigation; to Professor L. C. Dunn and Dr. Salome Gluecksohn-Schoenheimer for helpful advice and criticism; to Dr. K. W. Kressler and Dr. K. H. Slagle for the photographic technique, and to Mr. John Carty who was primarily responsible for the breeding of our albino stock for several years.

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A CRITICAL NOTE ON DIFFERENTIAL CLASS FECUNDITY IN BRAZIL

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A PAPER by J. B. Griffing, this JOURNAL 1940, 31:13-16 offers the conclusion that in Brazil economically superior groups are rearing more offspring than laboring groups, in contrast to the trend in the United States and in some European countries. Thus in 285 families from which originated students attending the agricultural college of Minas Geraes the average number of offspring was 8.2+ (6.8 living and 1.5 deceased), whereas in 54 families of laborers on the college campus ("complete" by the criterion of age of wife) the average number of offspring was 8.4 (4.8 living and 3.6 deceased).

Although these figures do, as Griffing points out, give evidence of a "terrific death rate among the lower classes," they cannot be taken at their face value in comparing the relative fecundity or mortality in upper and lower class families. This is because the families of the students were ascertained through interviewing offspring, whereas the families of the laborers were ascertained through

interviews with the parents.*

The method of ascertainment insures, for example, that the student sibships have produced at least one surviving offspring. Not so, in the case of the laborers' families. Moreover, of two families from the student stratum having the same number of offspring born, the family with the lower mortality rate is more likely to get into our sample, since nonsurviving offspring do not attend college. Hence families with high mortality are screened out, and the real difference in child mortality of the upper and laboring classes is almost surely somewhat less than that reported.

In the fecundity comparisons a basic correction is necessary which would inevitably reduce and might reverse the differential trend which the raw figures suggest. Weinberg,[4] Greenwood and Yule,[2] Cattell[1] and Lenz[3] and others have pointed out that when families are ascertained through offspring, the chances that a family will be "caught in the dragnet" are proportional to the number of offspring. For example, a six-child family is six times as likely as a one-child family to have a child that will qualify for a particular selective process at a particular time. In order to estimate the distribution of sizes and the average family size of the stratum represented by a population sample ascertained through offspring, it is necessary first to divide the frequency of families of each size s by s, i.e., the frequency of six-child families by six, of five-child families by five, etc.

The proportion of childless families should also be taken into account. Obviously none of the families producing the students was childless, but some of the laborers' families may have been. Following the correction of the average size of the students' families, the average for the laborers' families should be rendered comparable by limiting the families to those which were fertile.

One can only roughly estimate the magnitude of the necessary corrections in the absence of the original data on the distribution of family sizes. It is of interest, however, to note that in a study of family size recently completed at the Genetics Record Office, some "uncorrected" family size averages and corresponding averages "corrected" for ascertainment through offspring ran as follows:

AVERAGE FAMILY SIZE

Uncorrected	Corrected	No. families
7.5	5.8	117
6.7	4.9	114
6.6	4.5	63
6.2	4.5	63
5.5	3.9	106
5.2	3.8	107
4.0	3.1	52
3.9	3.1	53
3.9	2.7	61
3.8	2.8	52
3.3	2.4	102
3.1	2.2	93

It is seen empirically that the corrected averages tend to be about 72 percent of the uncorrected averages. If this should hold for the families of the Brazilian students, the average number of offspring per fertile family would be 5.9 instead of 8.2, well under the average for the laborers, 8.4 (which might be a little higher if childless families were allowed for). Uncertainty as to the true survival rate in the stratum represented by the students, however, prevents an estimate of the effective fecundity differential.

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*There is also a question of secular trends in mortality and fecundity. If child mortality and family fecundity are on the decrease in Brazil, the indices of both would probably be exaggerated in the sibships of the students as compared with the offspring of the laborers, who presumably are somewhat younger than the parents of the students.

ALBINISM IN MOCKINGBIRDS

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Avery Island, Louisiana

IN this JOURNAL for January, 1939, appeared a photograph of a pair of albino robins, accompanied by a short description. This somewhat sketchy account tells of several broods of robins hatched at Grinnell, Iowa, in each of which albinos occurred. There is no statement as to the color of the eyes of these "albinos." The photograph which illustrates the article gives the impression that the eyes are normal. Since red photographs black on orthochromatic film, this is not conclusive. If the eyes are pigmented the young white robins are not true albinos, but would resemble the non-albinotic white of the Wyandotte.

A more accurate history of these white robins should be recorded. It would be interesting to know if any of the white young survived, and it is most desirable to know if the eyes lacked normal pigmentation.

In my long and wide observation of birds, I have taken not less than ten white individuals belonging to as many species. Not one of these white birds were true albinos, as all had normal eyes. I have taken six mammals of normally pigmented species, whose pelage was pure white, but in every case the eye coloration was normal. But one instance of true albinism in the wild has come to my attention. An account of this follows:

On July 23, 1924, one of my office assistants, Mr. Terrell, called my attention to the fact that he had seen on his lawn, two white mockingbirds (*Mimus polyglottos polyglottos*) which came from a nest of four, the other two being normal in color, the brood having left the nest that morning. The nest was in a Yaupon (*Ilex vomitoria*) tree growing in his yard. I went with him and saw these two white birds and their normal nestmates being fed by their parents.

Both of the parents were normally colored. The young birds could fly, so I made no attempt to catch them, thinking that after they were fully feathered I would collect them for specimens.

The next day on looking for the white birds, they could not be found, although the two normal young were with their parents. This must have been the second brood raised by these two birds, as it is quite usual for mockingbirds to raise three broods in a summer—the first hatching between April 1 and the middle of May; the second between the middle of May and the end of June; and the third, during July.

The 1925 Broods

No further observations of white mockingbirds were made until April 19, 1925. Then, Mr. Terrell reported that a mockingbird's nest in his yard, in the same Yaupon tree in which the white birds had been hatched the year before. There were four young birds, one of which was white. On examination, I found this white bird to be an albino, with not a sign of color in plumage, beak, claws, or eyes.

The plumage of the three other young birds was normal, as were their parents. A daily watch was kept over this nest, and the young white bird removed and taken to my house just before it could fly. It was given the freedom of the sun porch, on which were growing a number of large potted plants. It soon became very tame.

The occurrence of this second brood of mockingbirds, one of which was an albino, hatched in the same tree as the white ones the year before, caused me to keep a careful watch on the two old birds, and I was rewarded by locating their second nest in a small Mandarin Orange tree about fifty feet from my office window, in which on June 29, there were two young—one normal, and one albino.

On July 7, these young birds were almost fully feathered, and the white one

was taken from the nest and sent to my house.

Further observation of this same pair of birds, which were easily identified by their tameness, located a further nest in which a brood of three was hatched on August 3. Two of these three were normal; and one was a perfect albino without sign of pigment.

I was now convinced that the parents of these three broods, in all of which albinism had shown during 1925, were the same birds that had hatched the two white young the year previously.

This last albino was taken from the nest and was sent to my house, but failed to live. The two previous albinos sent to my house proved to be both males. The one taken from the nest on April 19, began singing on June 3. The song was that of the normal young mockingbird, but the voice was weak. The one taken from the nest on July 7 began singing on August 24. Its voice was also weak. Both of these birds were perfect albinos and were identical in plumage, and in all outward appearances. Both were inactive during the brightest part of the day. It was evident they had difficulty in seeing clearly when the sun was shining or in normal daylight. In the early morning, before the light got bright, and in the late evening from just before sundown to dark, and on very cloudy days, they were very active. They were quite active also while the electric lights were on at night, as the brightness of electric light is far less than full daylight. During the bright part of the day these birds took no food. They took food eagerly, however, during the dim light of early morning and late evening, and by electric light.

After six and a half months in captivity, the albino taken April 19 escaped and flew into a large Live Oak near the house. Although a constant watch was kept on him during the rest of the day, he could not be recaptured, and was not seen the next day.

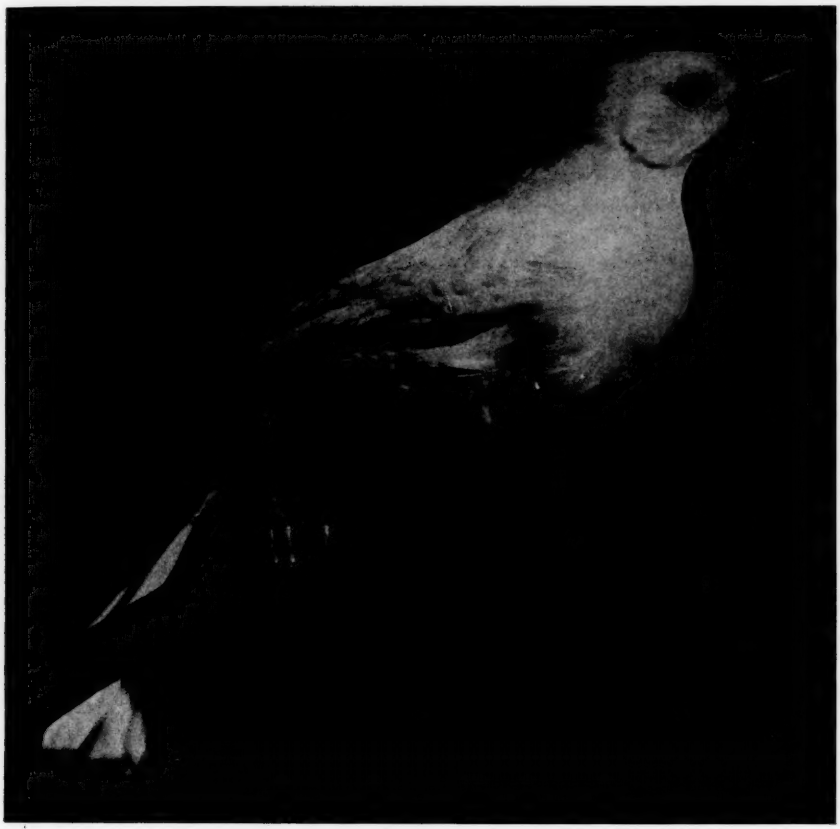
The second young bird became a great pet, and, as it was never really caged, soon became very tame. Its favorite food was insects and earthworms, but it also took various fruits—especially ripe figs, bits of apple, and bits of banana. I often got for him wasp nests of various kinds, and fed him the grubs and spiders from them. In the middle of the day, when I would chirp to him and put insects for him in the palm of my hand, he would fly to my hand, but had great difficulty in seeing the insects sufficiently well to pick them up. He would turn one eye and then the other toward the food, and raise his wings in the characteristic movement of a mockingbird when searching for insects in the grass, but did not seem to see well enough to pick up with accuracy the insects held in my hand. Late in the evening or early in the morning or after the electric lights were lit, his eyesight seemed to be almost normal—certainly very much better than in the middle of the day.

This bird had free use both of the glassed-in sun porch and of the screened living porch that open into each other on the south and west sides of my house. These two porches are 16 feet wide and have a total length of 90 feet, one is inclosed in glass and screen, and the other only screened. Both have many large potted plants on them, and among these plants my pet searched for insects, and found satisfactory sleeping quarters. He was happy and friendly, and would sit for a considerable time on my knee or shoulder while I stroked his back. He was almost eleven months old when killed by a police dog, with whom he had been most friendly.

It was quite evident that the nest of August 3, 1925, was built by the same birds who produced albino young in the summer of 1924, as they nested in the same tree used the previous year—a characteristic of mockingbirds.

Four Albinos in 1926

In 1926, what we believed to be the same pair of mockingbirds which had produced the albino young of 1924 and 1925 built a nest in late March in the same Yaupon tree in Mr. Terrell's yard where the nest had been built during the two preceding years, in which white young had been hatched. There were



AN ALBINO MOCKINGBIRD

Figure 4

Many albinos reported among wild species have pigmented eyes, and are thus not to be classified as true albinos. This bird has not a trace of pigment in feathers or eyes, as a genuine albino should. Of eighteen albinos produced by a single pair of mockingbirds not one survived to maturity, even though several of them were tamed and protected from natural enemies.

four eggs in the nest. The eggs were normal in coloration and were hatched on April 12. Three of the young were normal, and one was an albino. After leaving the nest, all four young were observed with their parents for two days, but during the afternoon of the second day the albino disappeared.

This same pair of old birds then built a nest in one of the Mandarin Orange trees between Mr. Terrell's house and my office. Four eggs were laid, all normal in coloration. This brood was hatched on May 19. Two of the young were normal; and two albinos. On the night of May 27, when the young were almost fully feathered, all four disappeared—probably taken by a snake.

A third nest for the year was built by this same pair of birds in the fourth tree in a row of Mandarin orange trees between my office and Mr. Terrell's house, and about fifty feet from my office window. This orange tree was less than

100 feet from the Yaupon tree in which the April 12th brood was hatched, and about 20 feet from the May 19th nest.

Wishing to protect these young for fuller observation, I had a frame built over two rows of orange trees adjacent to the tree in which the nest was built. This made a cage 22 by 45 feet, and 12 feet high, covered with heavy hardware cloth—four meshes to the inch. This made what I thought a vermin-proof cover over the eight orange trees inclosed. I left the door of this inclosure open, so that the old birds could go in and out and get used to the pen which was closed permanently when the young were hatched. Three young were hatched in this nest, two normal and one albino. The nest was started on June 21, the first egg laid on June 24, and the third and last one on June 26. All three normally colored eggs hatched July 7.

A plentiful supply of food was furnished, consisting of insects, earthworms, and fruit. The young birds after leaving the nest were left with their parents, and the albino observed to be deficient in sight. During the brighter part of the day, it could not see well enough to take food from its parents' beaks, when food was offered. Its voice and movements were somewhat weaker than those of the normal young. This albino was found dead on the ground four days after leaving the nest, with its head and beak bloody, showing the effects of severe pecking given it by the male bird, who had been observed fighting it. This was the last nest in 1926.

The 1927 Crop

The two normal young with their parents were kept in the inclosure, and went through the winter satisfactorily. The old birds began nest building in one of the orange trees on April 3, 1927. The first egg was laid on April 5, and the fourth and last on April 8. All eggs were normally colored. Four young were hatched on April 19—two normal and two white. Both of these white birds were albinos. All four left the nest on April 29 and were fed by their parents. It was noted that the old bird fought the two normal young from the last season's brood, who had been left with them the winter. It was also noted the albinos were weak in voice and sight, and were far more active early in the morning and late in the evening than in full daylight.

A second nest was begun on May 5, and the first egg laid on May 9. On the morning of May 9, one of the young albinos was found dead on the ground, its head and back badly pecked, and the other albino was also somewhat hurt. I removed this bird, but could not save it.

Four eggs were laid in this nest, all normally colored, the last one on May 12. On May 24, all four eggs hatched, two normal and two albino. They left the nest on May 31, and as soon as they were out of the nest, the parents showed great hostility to the previous brood, fighting them so constantly that I thought best to liberate the remaining normal young, as they could take care of themselves.

Realizing the parents resented their young being confined in the same inclosure with them, I caused to be built an enlargement and extension of the flying cage on the north side, 22×45 feet and 12 feet high, an exact duplicate of the first inclosure. Into this second inclosure, I put the four young of the last hatch as soon as a new nest was begun.

The third nest was started on June 16, and the first egg laid on the 20. Three eggs comprised this set, all normally colored, and were hatched on July 3. Two of the young were normal, and one an albino. These young birds left the nest on July 18, and later, as the old birds showed no inclination to build another nest, and as they seemed entirely happy with their three young, one of which was albino, I opened the two connecting doors between the two inclosures and let the family have the use of both.

As soon as the old birds entered the inclosure where the early group was, both the old birds began chasing and pecking the four young of the first group. The albinos being deficient in sight and ability were soon in a bad way. One of them was injured by flying against the

wire which necessitated its removal. The other albino, during the afternoon, was so badly pecked about the head, that it was necessary to remove it also.

These two injured birds were put in small cages. The one that had been hurt against the wire recovered within a couple of days, got out of its cage and flew away and was not seen again. The one pecked by its parents died.

I was then left with one albino from the last brood and four normal young birds. Fearing the parents would attack the albino, I closed them in the first flying cage, leaving the young in the second inclosure. About ten days later, I missed the albino and could find no trace of it or its feathers. A search was made, and in one of the orange trees a three-foot chicken snake (*Coluber guttatus*) was found, with a decided lump half way down its body, which proved to be the remains of the last of the 1927 albinos.

As I was not interested in raising normal mockingbirds, I liberated the four young birds and let the two old birds have the use of both enclosures.

The Experiment Ends

The two old birds seemed entirely satisfied with their inclosed home, and went through the winter in good shape. On March 27, 1928, the pair were observed nest-building in one of the orange trees in the first enclosure in which they had previously nested.

The first egg was laid April 2, and the fourth egg on April 5, and brooding began on that day. All eggs were normal in coloration. The young were hatched on April 16, two normal and two albinos. The young birds left the nest on April 27, and were taken good care of by their parents who fed them often, as they hopped about on the ground. The albinos were observed to be deficient in sight, except before sunrise and after sunset. In early morning and late evening they were far more active than their parents or normal nest-mates, but in full light were sluggish.

On the eighth day after the young left the nest, the mother bird started a second nest. The first egg was laid May 9, a clutch of four completed, and brooding began on the 12. All eggs were normally colored. The young were hatched on May 23; two albino and two normal.

The male bird had fed the young of the previous hatch while the mate was brooding, showing no hostility. Fearing from past experience that as soon as the second brood needed food the parents would fight their early brood, I inclosed the four young in the second apartment, as they could now feed themselves.

The second night after this separation, a heavy thunderstorm occurred, and one of the albinos was found dead the next morning.

The remaining three young grew normally, but the last albino of this group was swallowed by a two-foot Coluber on the night of June 7.

I then liberated the two normal young birds. The four young hatched on May 23 left the nest on June 2, and were taken good care of by their parents.

A new nest was started by the female on June 24 in an orange tree adjoining the one in which her last nest was built. The four young from the previous brood, two of which were albino, were now closed in the second compartment. The first egg in the new nest was laid on June 27, and the second egg on the 28, the third on the 29, and then brooding began, all eggs being normally colored. A few days later, as I had not observed the mother bird off her nest for two days, I investigated and found her dead in the nest, death having occurred at least twenty-four hours previously. Having no reason to keep her mate in the inclosure, I opened the gate, and he was soon outside chasing other mockingbirds in the vicinity. The four young hatched May 23, two of them being albinos, seemed to be happy in their inclosure, and were liberated in late August. The first day out, one of the albinos was caught by a sharp-shinned Hawk (*Accipiter velox velox*). The second disappeared.

Four days after being liberated, the old male had driven away the mate of a female, which had lately hatched a brood in a Live Oak nearby. These young had only been out of the nest two days,

and had spent most of the daylight time on the lawn in front of my office, where I watched their parents feeding them. Having driven off the male of this family, the recently liberated male joined the mother bird in feeding her brood.

Three days later I was greatly surprised to find this male of albino fame, who had been out of its inclosure only a few days, back in his inclosure with the captivated female, who had left her young and mate to begin housekeeping with the old male.

Nest building began at once, so that the gate to the inclosure was closed. Five days later the first egg was laid. The set was four, and hatched twelve days later. All the young were normal. No other nest was built during 1928.

I kept the old male and his new mate inclosed all winter, and on April 3, 1929, nest building was begun in one of the orange trees of the second inclosure. Four eggs were laid and hatched. All the young were normal. A second nest was built, four eggs laid and hatched late in May, and all these young were normal. A third set of three eggs hatched three normal young late in July.

I was now satisfied. Not having studied Mendelism at that time I considered that the albinistic trait was in the female of the first mating, so I opened the enclosure and ended the observations.

True albinism is almost universally inherited recessively, usually as a simple Mendelian character. In all, the pair of mockingbirds which produced albino offspring hatched a total of forty-three young, twenty-five normally pigmented and eighteen albino. According to expectation a simple Mendelian segregation should have given thirty-two normal and eleven albino. The excess of albinos is consistently maintained throughout the series. The sample is so small that the deviations may reasonably be attributed to chance. Since all of the broods reared during four years are recorded, the tendency present in pedigree analysis to select progenies having an excess of recessive offspring would not account for this deviation.

Further evidence that the character is a simple recession is furnished by the fact that the male mockingbird which produced albinos with one mate failed to produce them with another. True, the number of offspring was not large but what evidence there is points to the probability that this other female was not heterozygous for albinism. Consequently no albino young would be expected.

One of the most interesting features of this series of observation is, that from the normal young raised by the original pair of mockingbirds, which had produced albinos in every brood, not a single albino has been seen. While no actual identifying record could be kept of the nesting of these young birds, it is more than probable that they have nested in the vicinity in which they were raised. A careful check of every mockingbird nest in a radius of 500 feet from my office has been made during the past 10 years, and there has been not less than eight pairs nesting in this radius each year. There has not been an albino chick in any of these nests. This indicates that in this population, which gave rise to a pair of mockingbirds heterozygous for albinism, the gene is not common.

Summary

Both mockingbirds from whose mating albinos resulted were normal in color. Twelve nests, in which 43 eggs were laid in a five year period, were under observation. All eggs were normal in coloration. All eggs hatched except the last clutch of three eggs.

Of the forty-three young birds, 18 were full albinos, 25 were normal. There were no intermediates.

The normal young were normal in every particular.

The albinos were deficient in sight, weak in voice, and not as active on the wing as their normal nest-mates.

All normal young except two survived. No albinos survived. In nature the survival of albinos to maturity would be phenomenal.

The male bird after his original mate died, took a second mate and raised four broods, totalling fifteen young, every one of which was normally colored.

ACHONDROPLASIA IS ALWAYS HEREDITARY AND IS INHERITED DOMINANTLY

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(Director: Tage Kemp, M.D.)

THE etiology of achondroplasia (chondrodystrophia foetalis) is still unknown. Numerous reports have been published of its hereditary occurrence, and there are also many descriptions of isolated cases in families in which no previous case of the disease has been observed. This has caused earlier authors to suppose that achondroplasia may be either hereditary or the result of external influences; but none of them examined in detail the frequency with which these two possibilities may occur. Although many hereditary cases have been described, no agreement has been reached as regards the hereditary transmission of the disease. Most authors maintain that the character is more or less dominant, sometimes also recessive, especially in the more severe forms of the disease. This view was put forward by, among others, Hickmann[7] who, on the basis of a recent extensive study, considers that the slighter forms of achondroplasia (under which heading he and previous investigators include partial micromelia, brachydactylia, and abortive achondroplasia) are inherited dominantly, whereas, on the contrary, this hereditary tendency decreases in intensity when the manifestation occurs in large parts of the organism. From this reasoning he reaches the rather surprising conclusion that typical achondroplastic nanism is inherited recessively. This in spite of the fact that if the extensive literature of the subject is examined critically, it will be found that, apart from Bonnevie's treatise, which will be discussed below, there has been no case of achondroplasia, occurring within a family and diagnosed definitely, that cannot be explained by simple, monomeric, dominant inheritance.

Particular interest attaches to Kristine Bonnevie's investigations.[3] With very considerable material as a basis she concluded that the heredity of achondroplasia is simple recessive. Bonnevie described a large and very interesting family from Drangedal in Norway. She stated expressly that her investigations were purely preliminary and partly fragmentary, and also that the cases were only "very probably" achondroplasia. Bonnevie's supposition that achondroplasia is inherited recessively, has, however, been generally accepted, and is quoted in nearly all later works as a definite fact. Bonnevie herself, however, concluded her treatise with the following appeal: "Before the hereditary type to which achondroplasia belongs can be finally determined, further extended investigation of the present population of Drangedal is necessary, and further repeated examination of subsequent generations should be made at intervals of from 10 to 15 years."

I responded to this appeal in July, 1939, when I examined the families mentioned by Bonnevie. A reconstruction of Bonnevie's pedigrees made it possible for me to identify every member. Bonnevie stated that 13 persons were achondroplastic. Ten of these were alive in 1914 when she conducted her investigation but she only saw one of them, and no exact measurements were made. In 1939 seven of the ten were alive, and I succeeded in meeting, examining, and measuring five of them as well as two small individuals born after 1914. The seven examined were of small stature (height/height sitting in cm. of the six individuals being: 128/69, 131/70, 140/80, 149/76, 156/85, 164/?), though they were taller than typical achondroplastic dwarfs.

The typical symptoms of achondro-

TWO GENERATIONS OF ACHONDROPLASIA

Figure 5

The father's height is 103 centimeters (41.2 inches). This type of dwarfness which is dominant in inheritance is characterized by a body of normal size, but with the limbs markedly shortened and the legs bowed (see Figure 10). Other characteristic features of achondroplasia are a large head and a short broad hand; the root of the nose is sunken. The rather frequent recurrence of this inherited type as a mutation in Scandinavian populations suggests a possible way in which Scandinavian legends about gnomes may have arisen.

plasia are generally summed up as follows: dwarf growth (for men about 130 cm., women about 120 cm.), large head, prominent frontal eminence, root of the nose sunken, high palate, short extremities (especially shortening of the humerus and femur), arms so short that the tips of the fingers only reach the femoral trochanters, or a little over, 'main en trident' (short fingers, all about the same length, divergence between the 3rd and 4th fingers, short, broad hand), defective extension of the elbow-joint, very bowed legs, waddling gait, short broad feet, straight vertebral column and highly pronounced lumbar lordosis.

Not one of these pathognomonic symptoms of achondroplasia was found in any of the seven persons examined in Drangedal.

I was able to obtain information about the three who had died since 1913, and about the two whom I was unsuccessful in examining, in each case from their brothers and sisters; the present district doctor in Drangedal also provided me with some details. The statements all agree that none of the five was really small enough to be called a 'dwarf' and that the proportions and general appearance of all of them resembled those of the seven that were still alive. There is thus justification for saying that in 1939 there was not a single person in Drangedal with typical or unmistakable symptoms of achondroplasia, and that, in all probability, there were no achondroplastic dwarfs in 1914 either. The low stature of some of the population of Drangedal is probably due to rachitic changes or to the disease of Morquio; not one had symptoms of true achondroplasia.*

There was thus no recessive hereditary achondroplasia in this community, and it may therefore be asserted summarily that the literature of the subject contains no observation that contradicts the dominant inheritance of achondroplasia.

Pedigrees of True Achondroplasia

The investigations in Drangedal were conducted in conjunction with the examination of all the achondroplastic dwarfs in Denmark and a few characteristic cases in Sweden.

In Denmark, achondroplasia seldom occurs with several cases within a family; up to the present I have only discovered it in four cases, and in all four cases in two consecutive generations

Figure 6

(see Figure 6). All the other cases (39 so far) were isolated instances of achondroplastic nanism in large, normal families; in each individual case I possess information regarding every member of these families for several generations (as a rule four or five). Only two of these isolated cases of achondroplasia had solely children that were normal (Figure 7). The remaining 37 were childless.

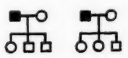

Figure 7

Thanks to the kindness of Professor S. Siwe, I had the opportunity of investigating in the pediatric section of Lund University, a very interesting family with achondroplasia (Figure 8).

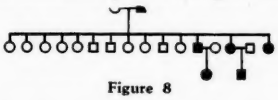

Figure 8

My own investigations (which are not yet concluded) include all cases, isolated and family, and enable me to form an estimate of the numerical relation between family and isolated cases within a circumscribed area of Denmark; these show a ratio of four family cases to 39 isolated cases, or 1:10.

*During my researches in Drangedal I was greatly assisted by Professor Bonnevie, who was kind enough to place at my disposal her material and all necessary information. Professor Mohr and Dr. Saeb were of assistance to me in many ways, and Dr. Pallesen-Mustikay, the district doctor in Drangedal, helped me during my examinations and accompanied me for several days on my visits to the inhabitants. For their valuable assistance I desire to express my deepest gratitude.

NORMAL FOETUS OF A DWARF

Figure 9

X-ray photograph of six-months foetus of achondroplastic woman. The arm and leg bones are normal in devolopement, and it can be predicted that the child will be normal. Achondroplasia is a dominant, and matings have in all cases been with normal individuals, so that half the offspring are expected to be normal.

LOCALIZED SHORTENING OF THE LIMBS

Figure 10

A "photographic chimera" giving a direct comparison between the normal body build and that of the achondroplastic dwarf. Note that the head and trunk of the dwarf are of normal size.

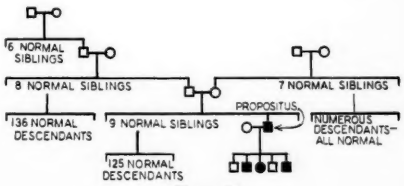

Figure 11

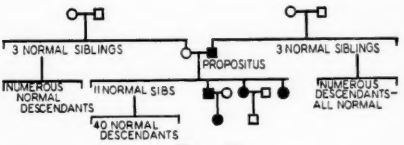

Figure 12

Many pedigrees, all, in fact, with isolated occurrences of the disease, reveal nothing as regards descent, but this does not exclude the possibility of these cases being hereditary, originating by mutation but not transmitted by propagation. The nature of the matter is such that no proof is possible, but since no difference between the definitely family cases and the isolated ones has been demonstrated regarding the origin, course, or symptoms of the disease, there is reason for supposing that both forms involve heredity. There is, of course, the possibility that the disease may be produced by exogenetic causes. In this connection foetal rickets or the other prenatal injuries have been especially considered.

Figures 11 and 12 are more detailed representations of two of the above-mentioned cases of undoubted family origin (Figure 6*D* and Figure 8).

The propositus in Figures 11 and 12 may be regarded as an isolated case in a normal family; if the propositus had been childless, the cases would have been very like the vast majority of those in Denmark, i.e. isolated, and the reason why the isolated cases are apparently not

hereditary is probably that these patients had no children.

This indicates that there is no reason to differentiate between hereditary and non-hereditary cases, since they may all be regarded as hereditary.

There is nothing in the above pedigrees to indicate that the disease can be inherited recessively. If this were the case, the pedigree in Figure 12 would show two conductors that were carriers of this very rare gene who had by chance married in three successive generations. No consanguinity can be demonstrated in any of the relations of the achondroplastic cases, neither can any sporadic cases be found nor evidence of the disease missing a generation, as might be expected in recessive inheritance. The heredity of achondroplasia must thus be assumed to be simple dominant, the disease occurring relatively often by mutation. This also agrees with the published observations of achondroplasia in twins; in the well-known case of the monozygotic pair (Schemensky), both suffered from the disease; in the dizygotic pairs (Hutchison, Rischbieth, Parhon and Schunda, Pritchard, Warner, own case) only one.

Achondroplastic Animals

Diseases are found in animals that are quite identical with achondroplasia in human beings; and in this connection the question is not one of dachshunds or similar breeds, but of the pathological conditions of individuals of definite breeds of animals which have otherwise shown none of the achondroplastic characteristics, i.e. the same conditions as those which affect human beings. Such diseases have been described more especially by Landauer and his co-workers,[8] Landauer and Dunn wrote on achondroplasia in fowls (Creeper-fowl), in which the disease may be produced by mutation and then show simple dominant inheritance with recessive lethal effect. Similar conditions probably obtain in Dexter cattle, the heterozygotes of which are short-legged but otherwise normal, while the homozygotes are very much deformed as the result of achondroplasia.

The total result of this investigation, therefore, may be said to indicate that achondroplasia (chondrodystrophia foetalis) in human beings is (1) hereditary and inherited as a single Mendelian dominant, and (2) occurs comparatively frequently by mutation.

Summary

It has hitherto been assumed that achondroplasia (chondrodystrophia foetalis) occurs in a hereditary, frequently recessive, and in a non-hereditary form. Critical review of the literature shows that there are no reliable observations that contradict dominant descent.

Re-examination of the apparently recessive cases in Drangedal, Norway, described by Bonnevie, showed that these dwarves were not achondroplastic.

Concordant achondroplasia was observed in a pair of monozygotic twins, and discordant in six dizygotic pairs.

A preliminary report is given of the author's own unfinished investigations in Denmark and Sweden. These make the following assumptions probable:

Achondroplasia (chondrodystrophia foetalis) is always hereditary and is inherited dominantly.

It is relatively frequently produced by mutation.

Similar dominant defects have been reported in chickens and cattle.

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METHODS OF SECURING BLOOD FROM RATS

As Developed in a Study of Blood Groups and Their Inheritance

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In order to insure a lasting source of known diagnostic serum for the determination and classification of blood groups in rats[1, 2] it became necessary to devise a method of securing as large a quantity of blood as possible from a rat without sacrificing the life of the individual. This was desirable so that an animal which is a source of diagnostic serum may be used many times to supply serum for testing other rats.

Attempts were made to obtain blood from rats by piercing with a small needle the jugular or femoral vein of the etherized animal. But the small size of these veins made it very difficult to insert the 26 gauge needle used, and the amount of blood secured was negligible. By cutting the tip of the tail it is possible to secure blood from the caudal artery and with considerable practice as much as 3 c.c. can be secured at one time.

The animal is etherized by placing it in a glass battery jar, to which several drops (about 1½ c.c.) of ether have been added. The cover of the jar is then weighted to prevent the escape of the rat. Before adding the ether a handful of shavings should be placed in the bottom of the jar to absorb moisture which accumulates. Following this method, at normal room temperature, a full-grown rat will become anesthetized in about two minutes. Some individuals, especially large ones, require considerably more ether and a longer period to become anesthetized; more ether may be added by sliding the glass cover off just enough to allow the pouring in of the necessary quantity. Furthermore, it has been observed that animals of some families are more readily anesthetized than those from other families, and hence there is danger of excess etherization in the former. The degree of etherization may be controlled by observing the condition of the animal.

When the condition of surgical anesthesia has been reached, as is apparent by the slumping of the animal to the bottom of the jar, the rat is removed and placed on a table. While it is in this condition, the tail is rubbed with a little xylol to cause dilation of the blood vessels, and to reduce surface tension. After the tail has been washed with 95% alcohol, an attempt to partially sterilize it, its tip is snipped off about ⅛ or 1/16 inch from the tip.

To insure continued anesthesia during this period the nose of the rat is placed in the end of a small bottle (Fig. 13*B*) to which has been added a little cotton and a few drops of ether. As long as the animal breathes regularly there is little danger of over-anesthetization. However, if breathing becomes short and in gasps the bottle should be removed until normal breathing is resumed. If the animal tends to withdraw its nose from the bottle, or shows signs of other voluntary movement, a few drops more of ether should be added. If a rat is over-anesthetized it may be revived by holding its nose between the index finger and thumb of the operator's right hand and blowing into its nostrils. The nose of the rat is thus brought close to the operator's mouth, a tube being formed by the operator's thumb and index finger through which tube the breath is forced. When the thorax of the rat has expanded, indicating that air has been forced into its lungs, the air may be expelled by squeezing the thorax of the rat with the left hand. This procedure is repeated until the animal resumes breathing spontaneously, or until there

seems to be no hope for revival, the heart having ceased to beat. This method of resuscitation has frequently been successful.

A few drops of blood may be obtained from an unetherized rat by holding it, with tail protruding, in a wire cone-shaped rack similar to the one shown in Figure 13*H-I*. The flow however is very slow and the tail must be rubbed with the fingers, like the stripping action in milking a cow, starting at the base of the tail and continuing to the tip. The more ready bleeding of etherized rats makes etherization well worth the extra time involved, particularly since this of itself causes no apparent harm.

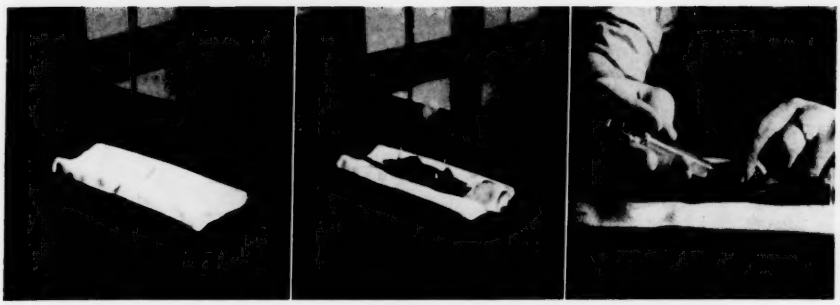
Cardiac Puncture

Even with the greatest precautions, blood obtained from the tail becomes contaminated, thus causing sores at the site of injection, when blood is introduced subcutaneously or intraperitoneally for the production of agglutinins in another animal. To obviate this difficulty and to find a more nearly sterile method of withdrawing blood, a cardiac puncture may be made. With a certain amount of practice it is possible to obtain readily, by this method, as much as 5 c.c. of blood without apparent serious injury to the animal.

To obtain blood from the heart, the rat is placed in the etherization jar, prepared as described previously, and when the condition of surgical anesthesia has been reached, it is removed and placed on a small towel into which a little shredded paper has been folded in such a way as to form a groove (Figure 13*A*) into which the animal will fit when laid on its back (Figure 1c*B*Q. When the animal has been thus placed, the nose of the rat is brought into a small bottle (Figure 13 *B*) containing a wad of cotton with a few drops of ether to insure continued anesthesia. Precautions against overanesthetization should be followed, as previously described.

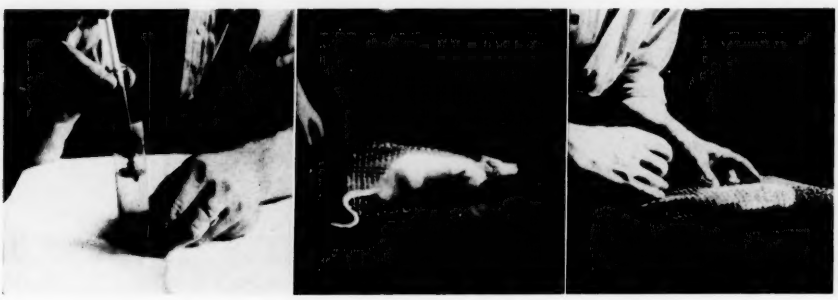
When the animal is thus arranged, placed with the head to the left of the operator, it is possible to feel the point of maximum palpitation of the heart by placing the left index finger over the fourth and fifth left ribs, with the thumb on the right side of the thorax (Figures 13*D-F*). The largest part of the heart normally lies between the fourth and sixth ribs. The needle should be inserted in this region, close to the index finger, at an angle approximately 45° with the horizontal and vertical axes of the rat's body (Figure 13*D*).* The glass syringe used in this work was a 2 or 5 c.c. Leur type; the needle 25 or 26 gauge, and ½ to ¾ in. long. Before the insertion of the needle a few drops of 95% alcohol should be poured on the skin just over the heart region, and the sterilized syringe rinsed in sterile citrate-salt solution (1% sodium citrate in physiological sodium chloride) so that the plunger will work freely. The barrel of the syringe is held between the right thumb and the fourth and fifth fingers, the index and middle fingers being used to grasp the plunger (Figure 13*C*), then the needle is slowly pushed into the thoracic cavity. In this position the throb of the heart may be felt against the end of the needle and after the penetration of the wall of the left ventricle blood will flow in spurts into the syringe, but when the right ventricle is punctured the blood will not flow as strongly. With a little experience it is possible to tell whether the left or right ventricle has been the source of the blood drawn. Occasionally an auricle may be punctured, in which case the blood will not come in spurts, but will flow evenly into the syringe as the plunger is pulled up with the index and middle fingers of the right hand. Failure to obtain blood will result when the needle has not punctured the wall of the heart, or possibly has penetrated just the tip of the ventricle, this position being indicated by a tugging sensation at each contraction of the

*After developing this technique the author learned that cardiac puncture is used in some laboratories to obtain blood from Guinea Pigs. In that animal the technique, especially the angle at which the needle is inserted, is quite different.



A—Groove in towel to hold rat. B—Etherized rat, nose in ether bottle. C—Position of syringe for bleeding.

D—1 cc. of blood removed. E—2.5 cc. removed. F—5 cc. removed.



G—Adding salt-citrate solution. H—Wire rack used to hold rat. I—Injection intraperitoneally.

HEART-PUNCTURE TECHNIQUE

Figure 13

Method of securing blood from a rat by means of a cardiac puncture, the mixing of the blood with the anticoagulant salt and sodium citrate solution, and the method of injection intraperitoneally of whole blood for the production of agglutinins. Photographs by the author and Paul Knight.

heart. In the latter case the needle is slowly drawn back away from the heart, but not withdrawn from the thoracic cavity, and after being pointed more anteriorly, is reinserted. One little spurt may indicate that the needle has been pushed entirely through the heart and is pressing on the tissue beyond, or possibly that one of the large blood vessels other than the heart has been hit. In a large rat it is at times necessary to press with the left thumb against the right side of the thorax to push the heart close enough to the left side so that the needle may enter it; a longer, (¾ in.) needle may be satisfactorily used for the largest rats. If no blood appears in the syringe, it may be necessary to remove the needle, and after being a little more positive about the point of maximum palpitation of the heart, to try another position. As soon as blood appears in the syringe, the plunger may be slowly pulled up, using very little force.

In some rats it is possible to remove 5 c.c. of blood in about 20 seconds, but under ordinary conditions it takes from 40-60 seconds to obtain this amount (Figure 13*C-F*). If too much force is used in the withdrawal of the plunger a part of the wall of the heart may be pulled against the opening of the needle thus blocking it. When blood is withdrawn too quickly the animal will often attempt to breath more rapidly and, occasionally, it has been noticed that the heart may beat rapidly for several times and then cease temporarily, later to continue normally if no blood is withdrawn for a time or if some of the blood in the syringe is forced back into the heart. Over-anesthetization, generally resulting in gasping and slow heart beat, can be overcome by removal of the etherization bottle; in this case it is almost imperative to cease attempts to remove blood until regular breathing and steady rate of heart beat has been resumed, for its removal at this low ebb of life has resulted, in several instances, in the death of the individual. During the bleeding process, the removal of the ether bottle may be desirable if there are indications that the rate of breathing is being reduced, and if the blood flow is sufficiently fast, the relaxing effect of the ether already acquired will be sufficient to keep the animal quiet for the remainder of the operation. Until the rat is thoroughly over the effects of the ether, it is desirable to keep it in a warm place away from drafts.

By the above method, it is possible to remove 5 c.c. of blood from a mature rat once a week for a period of three months without apparent injury or loss of health. Blood volume was not replaced by injection of salt or other solutions in any of these experiments. Some animals have had litters and raised them while on this bleeding schedule. As much as 12 c.c. of blood has been removed at one time from a 380 gm. rat without killing it.

Summary

A method is herein described for the securing of blood from an etherized rat by snipping the tail and by the more satisfactory and sterile method of cardiac puncture. Each of these methods may be done repeatedly over a long period of time with the same rats without serious injury.

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COLD STORAGE COLTS*

V. R. Berliner, F. E. Cowart and L. L. Pharis

On November 29, 1939, a mule colt, and on March 20, 1940, a horse colt were born at the Mississippi Experiment Station. Their arrival heralded the achievement of a goal long sought by many reproduction physiologists and horse breeders, for they were the product of artificial insemination with stored, 24-hour-old semen. These blessed events culminate much experimental work and demonstrate that in workstock production too, we have come close to the use of "long distance" breeding. The birth of two colts might not sound very spectacular at a time when numerous artificial insemination associations for dairy cattle are successfully in operation, using old semen as routine procedure, and when long range paternity is frequently achieved in calves and lambs. Only, in horse stock and in swine, it seemed for a long time as if the difficulties were too great and the situation too complicated for the successful use of stored semen.

This would seem peculiar, especially if one remembers that since the pioneer work of Iwanow in Russia, artificial insemination was first—and widely—practiced in horses, long before it was utilized in the other types of farm animals.

Neuman[7] discussing the achievements of five years' work of artificial insemination of farm animals on a large scale and in breeding cooperatives, pointed to difficulties in horse breeding that were due chiefly to two reasons: (1) the long heat period of mares, causing a variable and long insemination-fertilization interval, so that it becomes almost impossible to "hit" with safety the optimal time for insemination; and (2) the low viability of stallion sperm, during storage *in vitro*, and in the mare after insemination.

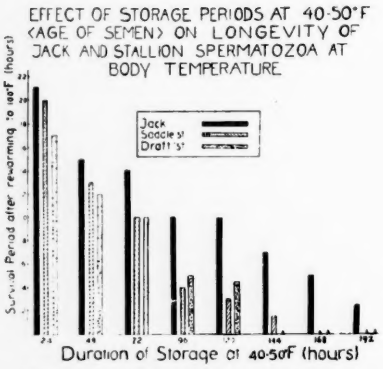
Selivanova[8] and Milovanov[6] found that ram and bull sperm have a greater resistance than stallion sperm, and Andreew[1] observed that ram and bull sperm survive in the female genital tract for 48 and 24 hours, and more, respectively, while stallion semen perishes in less than 20 hours.

Milovanov[6] believed that the low viability of stored stallion sperm was due to a toxic accumulation of metabolic breakdown products of the semen proteins, and to the unfavorable effects of voluminous secretions of the secondary sex glands preventing the onset of anabiosis. This was later confirmed by Shergin.[9] On removing these fractions of the ejaculate by centrifuging and by replacing them with an anabiotic dilutor, TGL-5, Milovanov was able to store stallion semen up to five days. This method was later successfully used by Walton[10] and McKenzie, Lasley, and Phillips.[5]

In our own work[2] using the semen from a jack and a saddle stallion, we were able to keep the sperm alive for up to 192 hours without centrifuging, because the ejaculates of these two stud animals contained but little accessory fluid, in contrast to the large amounts found in the ejaculates of draft stallions. In these experiments, Milovanov's[6] dilutor, TGL-5, was used, and it was found that a semen:dilutor ratio of 1:1 gave the longest storage periods and highest survival percentage.[3] Essential was a gradual cooling to 40°-45°F, obtained by immersing the containers with the diluted semen first into a jar with water of about 60°F, after which it was stored in a refrigerator at a temperature of 40°F.

Semen stored in this manner showed a high degree of motility for several days. However, when it was used to breed several mares, through only one insemination with doses of 20 cc. containing approximately two billion sperm,

*Contribution from the Department of Animal Husbandry, Mississippi Agricultural Experiment Station, State College, Mississippi. Published with the approval of the Director, Mississippi Agricultural Experiment Station. Paper No. 33, New Series.


EFFECT OF STORAGE PERIODS AT 40-50°F
(AGE OF SEMEN) ON LONGEVITY OF
JACK AND STALLION SPERMATOZOA AT
BODY TEMPERATURE
Jack
Saddle s'
Draft s'
Survival Period after rewarming to 100°F (hours)
Duration of Storage at 40-50°F (hours)

STORAGE REDUCES VIABILITY

Figure 14

Stored semen when re-warmed shows reduced survival with increasing length of storage.

the results were disappointing. All the mares were in full heat but in only one case was pregnancy obtained with 24-hour-old semen, resulting in the colt born on March 3, 1940. In this mare ovulation occurred within 12 hours after insemination, as had been calculated after palpating the ovaries. The other mares were bred regardless of the stage of the ovaries and had ovulated more than 24 hours after insemination. This outcome of the test was not surprising if one considers that even with natural breeding repetition increases fertility.

In the case of the mule colt mentioned at the beginning, the mare had been bred on two successive days, both times with jack semen that had been collected 24 hours before insemination. Later on, two mares, both in foal heat the same day, were inseminated with fractions of the same ejaculate from the jack that again had been stored for 24 hours, and they were also reinseminated with fractions from the original semen on the two following days. Both these mares became pregnant, as detected by pregnancy tests and physical palpation.

This beneficial effect of multiple insemination is not surprising; for even in cows and sheep one obtains a higher percentage of pregnancies and twinnings if two or more inseminations are performed,[1] although one would be inclined to believe that with the long survival period of bull and ram sperm one single insemination would be sufficient. The picture in case of the stallion and jack semen becomes more clear through a series of observations made by Cowart[4] on stored stallion and jack semen used for the breeding experiments. As shown in Figure 14, he found that if stallion and jack semen is brought back to and kept at body temperatures, the survival period becomes shorter the longer the semen had been stored before rewarming. This decrease of the survival period is shown in the accompanying chart. Therefore, it is safe to assume that, also after insemination, stored semen will survive for a shorter period in the female genital tract the older it was before it was used, hereby decreasing the chances of fertilization.

The case of the two mares is of practical interest insofar as it shows that it is safer to use fractions of the same ejaculate on successive days, if one can not obtain a new shipment of semen, than to use it all in one dose; because in this manner the survival period of the sperm in the mare might be too short to bridge the insemination-fertilization interval, whereas the cold storage semen when introduced on the second service may survive to ovulation. Of course, if one can time accurately both processes by palpating the ovaries to determine approximate onset of ovulation, one single injection may bring results, as it was shown by our colt, and also by the experiment of McKenzie and co-workers.[5] Also with semen collected less than ten hours before insemination, one single insemination can be successful as was found in routine breeding work here, but it is far safer to give several injections.

Since the demand for shipped semen of jacks and stallions increases continuously, we believe that the described breeding experiments have shown a way to use "cold storage" semen with such success that it will be a means to intensify the utilization of valuable jacks and stallions.

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✦

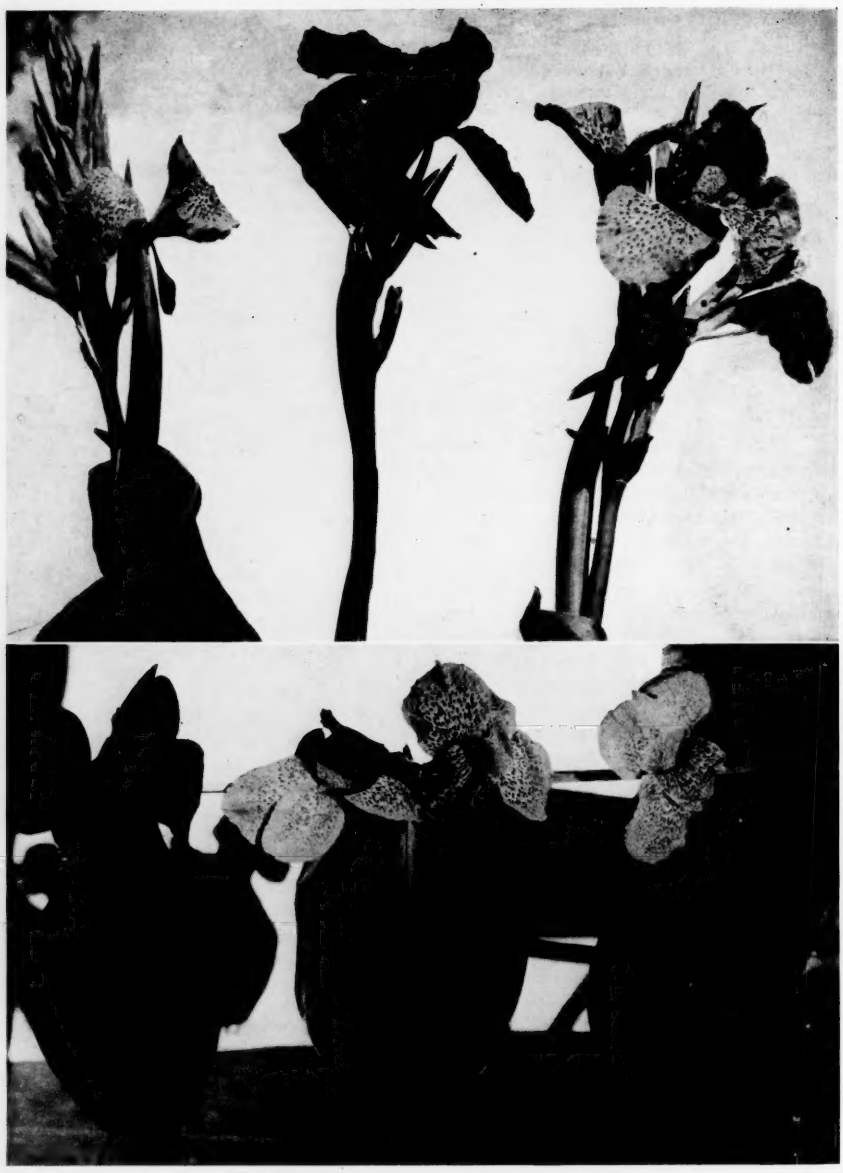
The Botanist's Microscope

THIS is a convenient book on botanical microtechnique,* exclusive of photomicrography and paleobotany. Convenient, because there are encompassed within the confines of a 523-page volume critical evaluations of apparatus, reagents, stains, methods, and procedures; information concerning sources of materials; suggestions for the treatment of groups in the several plant "phyla"; rules of deportment for the technician and encouragement for him confronted with difficulties. The book is illustrated with a frontispiece and 110 text figures, mostly photomicrographs, without magnifications given. Illustrations showing chromosomes are pretty well restricted to two photogenic and cytologically facile subjects, *Trillium* and *Lilium*. Perhaps less stellar performers sometimes demonstrate more patently the success or failure of a given technique. The bibliography, really the "literature cited," consists of about 250 references, some of them taxonomic; so, in view of the scope of the book and the rapid progress in the field, the citations are rather few. The index seems good.

Doctor Johansen should be well qualified to write such a textbook: he has had "some sixteen years of extensive personal experience in collecting materials and preparing slides of plants from over the entire range of the plant kingdom" (preface). And I think he has produced a practical and valuable book. All microtechnical workers in botany should have access to it. No doubt one may consider that certain "pet" methods have been omitted or inadequately presented; this is to be expected from any treatment, necessarily selective, under the title *Plant Microtechnique*. But, nevertheless, one may legitimately differ with the author in his general attitudes and opinions. For instance: I feel that, for an avowedly technical book as this is, information is often too naïvely imparted, as: "Bacteria, which are to be found everywhere under the most extreme of conditions, constitute a very low or primitive type of plant life . . ." (p. 211), a matter of general knowledge which people engaged in microtechnique ought to be acquainted with. Nor do I agree with the statement: "The common practice of attempting to learn microtechnique and to investigate a cytological research problem simultaneously cannot be too strongly condemned" (p. 170). And I can only commend to the "Raised Eyebrows Department" a strange recommendation that "one should be patient and cordial in explaining matters, as it frequently happens that the most unexpected favors are bestowed" (p. 204). However, this is the best text now available in this field.

J. T. Baldwin, Jr.
University of Michigan

*Johansen, Donald A. *Plant Microtechnique*. Pp 523. Illustrated. $4.50. McGraw-Hill Book Company, Inc., New York. 1940.



SECTORS IN FLOWERS AND LEAVES

Figure 15

The upper photograph shows three stems which came from the same bulb. The left stem carries a narrow red sector, the middle one is all red and the one on the right is practically all green except the left flower which contains a red sector.

The lower photograph shows (left) a flower and the leaf from the red sector, (right) a flower and leaf from the green sector, while those in the middle contain both sectors. Note here the sectors in both the petals and the leaf parallel the veins. The flower on the left is the one substituted in Figure 16.

A SECTORIAL CHIMERA IN THE CANNA

CHARLES DALLAS BLACHLY

VARIATION IN SIZE OF SECTORS

Figure 16

As no completely red flower of the chimera was available at time the picture was taken a red flower from another variety of canna was substituted for the flower on the left.

THIS sectorial chimera resulted apparently from a gene mutation in the bulb. The plant is comparatively well established as is shown by the fact that some of the bulbs were obtained from a local garden where they had wintered in the ground for four years. The gardener was not aware of their unusual nature. The plants grow to a height of about five feet. Some individual plants are similar to the one reported by Dr. Pope in this JOURNAL in January 1919.

This chimera is of special interest because certain red pigmented cells enable one to study cell division and the behavior of cells of two different heritages growing side by side in the same organism. The paths of the descendents of each of these different types of cells composing the sectors are easily followed. In the stems and leaves this red pigment produces a bronze color.

In most plants the bronze sectors are very narrow. Occasionally, however, there are plants in which the bronze sector spreads so much into the green, as the plant grows, that in the distal parts the green sector is completely crowded out. The sectors usually parallel the veins in the stalk, leaves and petals. Only occasionally will the bronze cells invade the regions of the veins, that, lower in their course, were in the green sectors. This parallelling of the veins is similar to that found in the fig sectorial chimera (this JOURNAL, January 1919). This will be considered later.

As indicated above, the microscope shows the color of the bronze sector to be due to a red pigment within the epidermal cells. In this it is similar to other varieties of bronze cannas and suggests that this type of mutation may be common in cannas. Hereafter the bronze portion will be spoken of as the red sector. This red sector may show only on one side of the leaves or petals or it may extend through the entire thickness.

The epidermal cells of both sectors are the same in size and distribution as are the stomata. In the younger leaves no break is seen in the arrangement of cells or tissues where the two sectors join. Nevertheless, the two sectors are more or less toxic to each other as is shown by some of the older leaves where a line of dead tissue frequently appears at the junction.

The petals of the red sector are red while the petals of the green sector are yellow, mottled with small, oblong, dots. For convenience we will call the mottled

petals yellow. Some of the flowers are completely red; others are completely yellow. Some individual flowers possess both completely red and completely yellow petals while other petals may possess both red and yellow sectors. (See Figure 16.)

The red sectors grow to be somewhat longer than the green. This is more evident in the margins of the petals that contain both sectors. Here the red sector may extend a quarter of an inch or more beyond the yellow, making a deep indentation at the margin where they join. In petals containing both sectors the extension of the red beyond the margin of the yellow is abrupt, showing that the growth factor of the red probably does not diffuse through the plant tissue in its entirety but remains in the red.

As mentioned above the sectors parallel the veins of the canna chimera, as is also found to be true in the fig chimera in which the sectors parallel the stem. The sectors do not follow the phyllotaxy of the axillary buds, which form a beautiful ascending spiral, independent of the sectors. This brings some of the buds of the spiral into one sector and some into another. In some twigs this axillary bud arrangement spirals to the right, in others to the left. Parenthetically, this arrangement of righthandedness and lefthandedness is seen perhaps to the best advantage when two selected twigs of the same size from the lombardy poplar are properly oriented and held side by side. The one forms almost a perfect mirror-twin of the other. This strongly suggests that the atomic groups which determine the position of the axillary buds in the stems have a polarity corresponding to the course of the spiral. On the other hand it would appear both the veins and the cells are laid down by the same organizing structure which differs, as said above, from that initiating the axillary bud pattern. The paralleling of the veins and the sectors is well illustrated in the middle leaf of Figure 15. Figure 15 shows three stems which came from the same bulb. The left stem carries a narrow red sector, the middle one is all red and the right is practically all green except the left flower which contains a red sector.

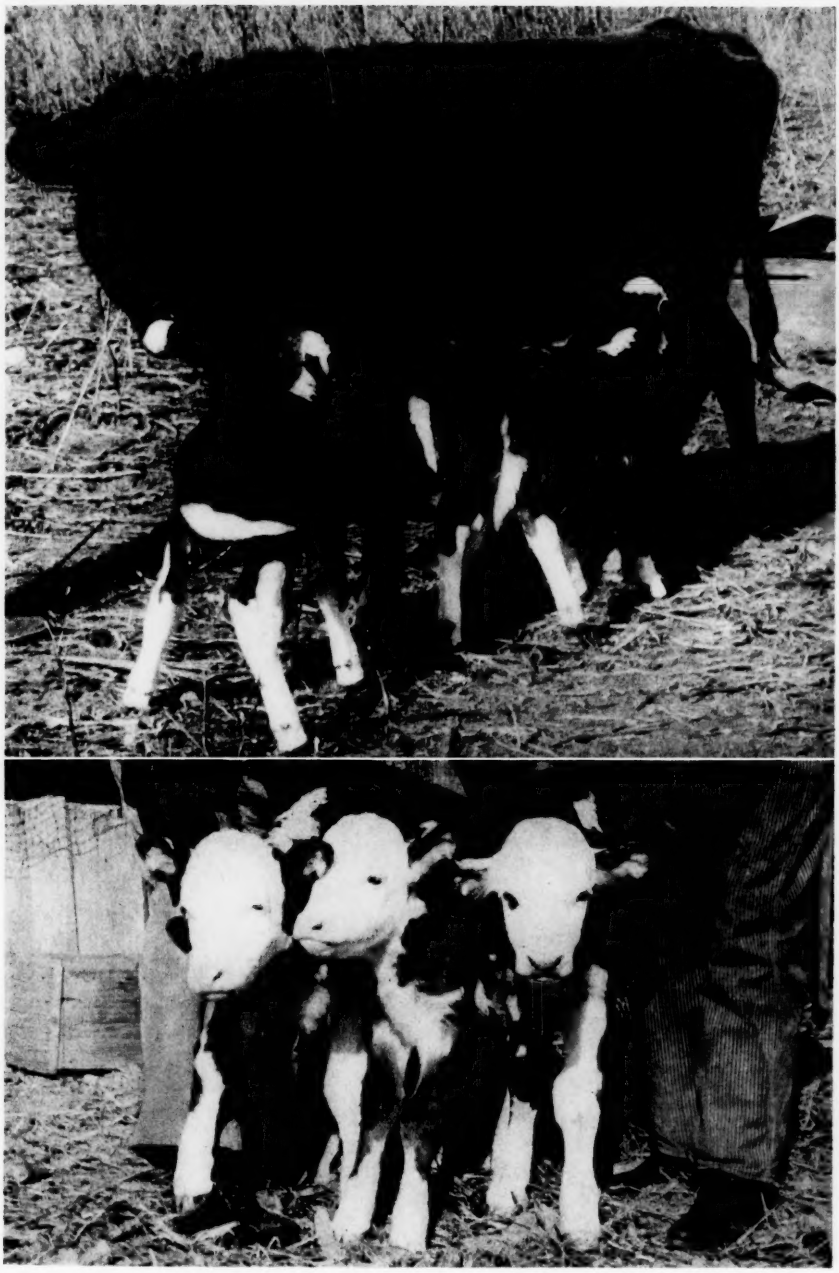
Are They Identical?

MORLEY KETCHUM is a farmer living at Vinita, Okla. His red cow Hardy was triply blessed on the cold night of February 2, 1940. Living triplets are by no means common in cattle, and identical triplets are much more rare. This picture caught the editor's eye because the markings of the three calves were so very similar. Each of the calves has a white face, and the red spots on the chest are almost identical (see Figure 17). The same amount of tail-tip is white in all three infants. It was something of a shock to learn that two of the calves are males and one female.

The heifer is on the left, facing the camera. Such mixed multiple births in cattle should constitute interesting material for the study of hormone balance in the development of secondary sexual characters. In this instance we might expect the heifer to develop into a "super-freemartin." Another case of triplet calves — two heifers and a bull — which were raised to maturity, recently came to the writer's attention. The heifers, apparently normal, did not conceive. The fertility of the bull was not established before the trio was slaughtered. In that instance a female preponderance appeared to have been unable to redress the balance enough to avoid the disastrous effect on the female bovine economy of developing as twin to a male.

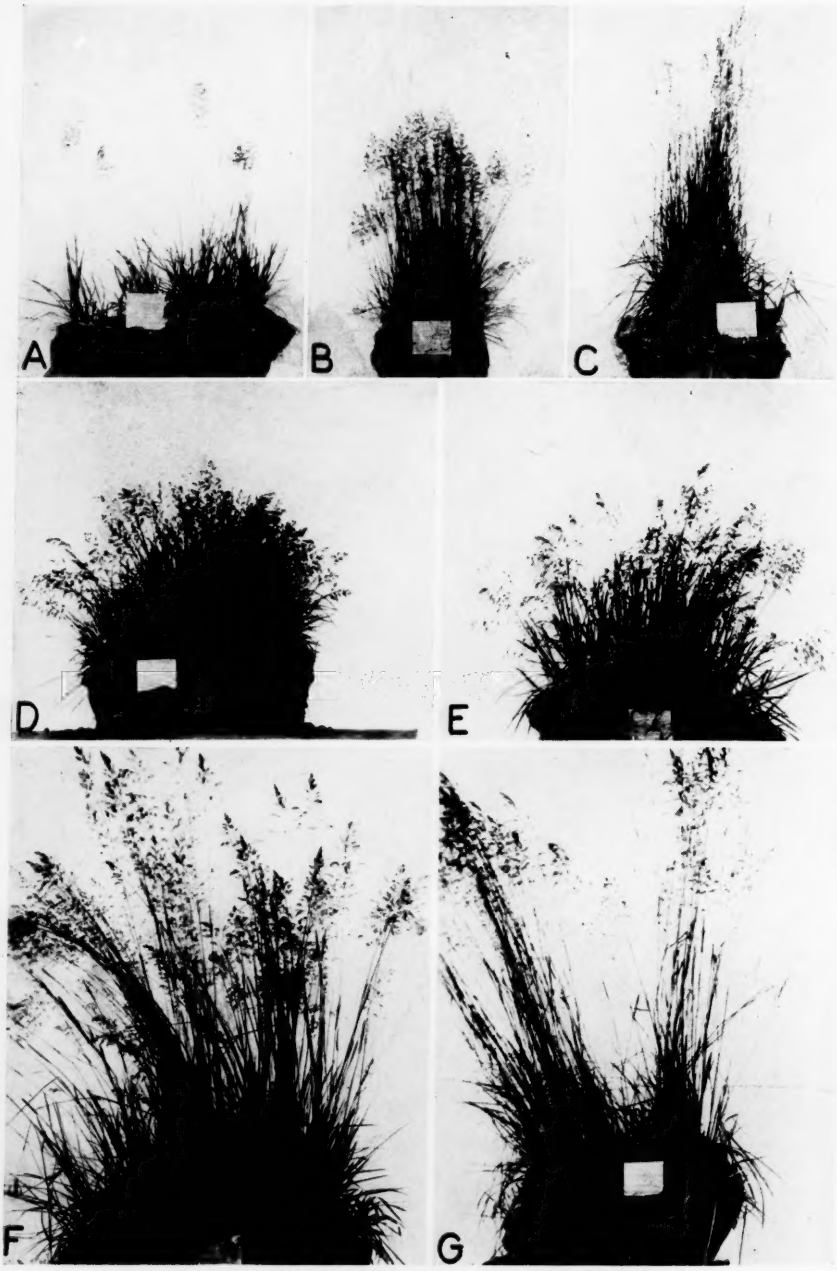
The fertile Australian "freemartin" reported in the last issue of the JOURNAL shows that there are cases in which the balance of development is maintained—either by the failure of the blood-vessels to anastomose, or for other reasons.

—R. C.



SIMILAR IN PATTERN BUT NOT IN SEX

Figure 17



RANGE OF VARIABILITY IN GROWTH HABIT

Figure 18

Striking differences in habit of growth are found in individual plants of Kentucky blue grass (*Poa pratensis*). This enormous variability gives the plant breeder an excellent opportunity to select types of agronomic value. Since this species reproduces largely by apomixis, such asexual progeny of a given plant are exactly like the parent type. By this means seed stocks of desirable types may easily be obtained.

THE PROGENY TEST AS A MEASURE OF THE TYPES OF SEED-DEVELOPMENT IN POA PRATENSIS L.*

FRED W. TINNEY† AND O. S. AAMODT‡

STUDIES on *Poa pratensis* L. by Müntzing.[8] Nilsson[10,11,12] and Akerberg[1,2,3] have shown that among the plants native to Sweden many different morphological types are found. Furthermore, plants grown from seeds collected from single open-pollinated panicles are strikingly uniform in morphological type and are practically identical with their seed parent. Müntzing suggested that the seeds developed by apomixis. Nilsson and Akerberg found that occasionally plants differ in morphological type from sister plants typical of the progeny, and they suggested that these atypical plants arose as a result of gametic union.

Little information is available on the method of seed-development in the types of *Poa pratensis* that have become established in North America. Kemp[7] mentioned the uniformity of single plant progenies in material collected in Maryland, and Armstrong[5] found that certain biotypes collected in eastern Canada were likewise uniform in type. Neither of these investigators, however, determined the method of seed-development.

A study by Tinney[13] of the cytology of certain biotypes collected in Wisconsin showed that after the macrospore mother cell divides by meiotic divisions to form haploid macrospores, all four macrospores disintegrate. Then a typical 7-celled 8-nucleate embryo sac develops without meiosis from a somatic cell of the nucellus. The embryo develops from the egg by diploid parthenogenesis. Under these circumstances a progeny from a single plant possesses the same genic complement as its parent, and naturally, therefore, the individual plants are alike in morphological characters. A review of the literature on the cytogenetics of *Poa pratensis* has been given by Tinney[13]. Since Tinney's paper was sent to press, Akerberg,[4] Brown, [6] and Müntzing[9] have published studies of the cytological behavior in *Poa*.

It is not practical to examine cytologically many plants in order to determine the prevalence of parthenogenesis. This information is obtained, however, by growing populations of plants originating from open-pollinated seed collected from single plants, and by rating the plants for uniformity. The fact that a progeny is uniform throughout and practically identical with its seed parent indicates that the plants arose apomictically or by the less likely process of self-pollination. The presence of atypical plants in an otherwise uniform progeny indicates that these plants may have arisen as a result of gametic union or mutation.

Materials and Methods

The material for the study described herein was originally obtained from Wisconsin, Missouri, Virginia, eastern Canada, Norway, Sweden, Denmark, Eng-

*Contribution No. 153 from the Department of Agronomy, University of Wisconsin, Madison, Wisconsin, in cooperation with the Division of Forage Crops and Diseases, Bureau of Plant Industry, U. S. Department of Agriculture.

†Dr. Tinney lost his life in the sinking of the S. S. *Athenia* on September 3 or 4, 1939, while on his return from the Seventh International Congress of Genetics, which met at Edinburgh, Scotland, August 23-29. He was assistant agronomist, Division of Forage Crops and Diseases, Bureau of Plant Industry, United States Department of Agriculture.

‡Formerly agronomist, Wisconsin Agricultural Experiment Station, now head agronomist in charge, Division of Forage Crops and Diseases, Bureau of Plant Industry, United States Department of Agriculture.

land, India, and the Union of Soviet Socialist Republics. The collections in Wisconsin were made from pastures in different parts of the State by harvesting separately single inflorescences and by isolating pieces of sod from which single plants were separated. Pastures were selected which showed no evidence of recent cultivation and which represented different soil types. In many instances the pastures were 50 to 75 years old. Collections were made both on pastures that were obviously grazed closely throughout the summer and on those that were grazed lightly. The material originating from outside the State was received as bulk sample lots of commercial seed. Individual plants were grown in the nurseries as single individuals from which seed was collected.

Each seed sample was germinated in sterilized soil in small bread pans. After the seedlings were well rooted they were transplanted into greenhouse flats, each seedling within the confines of a paper band containing sterilized soil. Later, usually in May, the individual plants were transplanted to the field in rows four feet apart with plants two feet apart in the row. There was a great advantage in using a field that had been summer fallowed the previous summer. The soil was relatively free from grass seeds, and the accumulated moisture supply made it unnecessary to water the transplants. The plants were examined for uniformity of type when all were in flower during their second summer in the field. Because all seeds available were planted, the number of plants or clones varied with each progeny row or plot.

Observations

One of the most striking characteristics of *Poa pratensis* is the range of variability in morphological types (Figure 18 *A* to *G*), and it is interesting that this variability of types may be found in the same pastures, although the tall, upright ones are usually predominant even in closely grazed pastures. The collections made to date in pastures in Wisconsin do not indicate that grazing or other environmental factors have been instrumental in the selection of certain morphological types, nor in their survival values. This also appears true for material collected in other States in the central part of the United States as shown by 18 samples of seed harvested by commercial seed companies in different parts of Iowa, Missouri, Kentucky, Wisconsin, Minnesota, Kansas, and Nebraska. The climate, especially rainfall, in the different localities in these States varies considerably, yet the different samples used in these studies from these localities did not differ in composition of morphological types or in degree of drought resistance. This uniformity of type is a marked contrast to the wide differences found in some of the species (*Bromus, Agropyron, Bouteloua,* etc.) in which the seeds arise as a result of gametic union, and which are common to the same region where the *Poa pratensis* was collected.

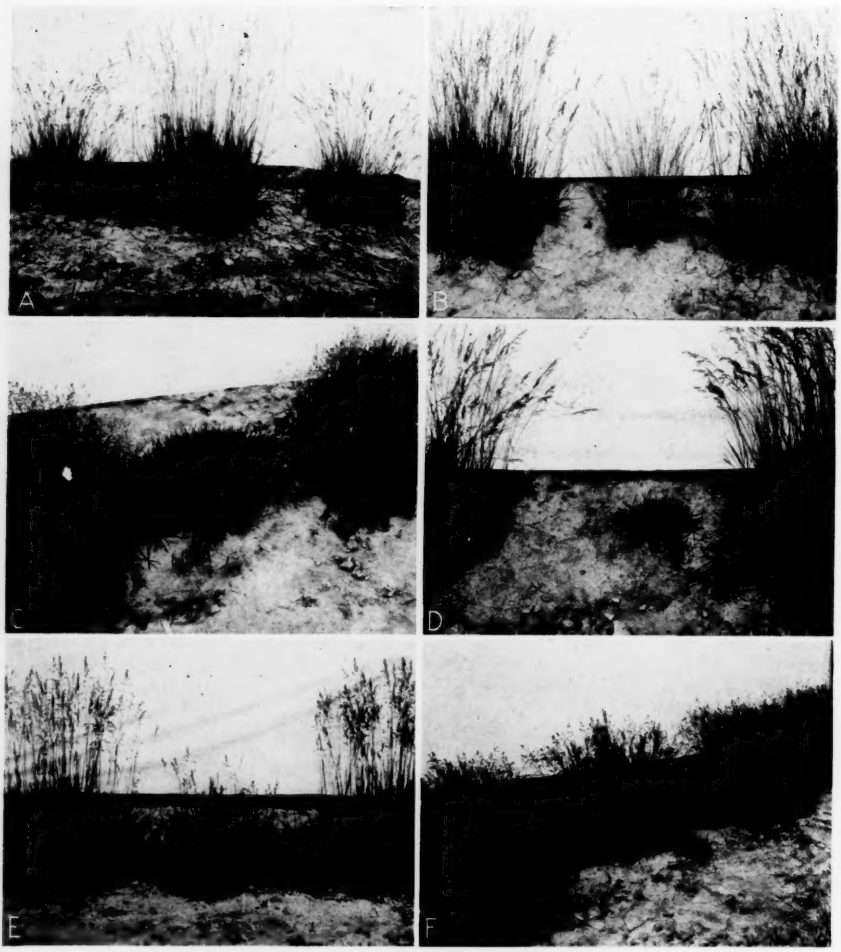
The progenies grown from seed collected from individual plants show, with two exceptions which will be discussed later, extreme uniformity (Figure 19, *A, B, C*). The number of plants, the number of variants, and the percentage of variants is shown for each progeny in Table I. In some instances the appearance of individual plants is affected by injury due to insects, diseases, soil erosion, or other mechanical agencies. Extreme caution was exercised in handling the seed and seedlings in order to reduce the possibility of mixing the plants in planting. The variant types that were counted, a few of which are illustrated in Figures 20 and 21 do not appear to be due to injury or errors in planting since these particular types are not to be found elsewhere in the nursery. In each photograph, end plants represent typical plants of the progeny line of which the plant in the center is a variant. Its appearance suggests that it consists of a different genetic constitution. In some instances the variant plant is taller (Figure 20*A*) and more vigorous, and in other instances it is smaller (Fig. 20*B-F*, and 21*A*). In Figure 21*B* the plant on the left is typical of the progeny, the one on the right is similar in vegetative habit, but

VARIANT AND UNIFORM PROGENY

Figure 19

Different plants behave differently with respect to the amount of apomictic seed produced. Progenies shown in *A*, *B*, and *C* are extremely uniform. In contrast to these, progenies shown at *D* and *E* had a percentage of variant types of 21.92 and 12.06 respectively. The occurrence of the variant types gives the plant breeder opportunity to extend the range of selection beyond that available in the true-breeding apomictic lines.

lighter green in color and has longer inflorescences. The dwarfed plants shown in Figure 20*C-D* produced no inflorescences, while another shown in 21*A* shows less growth, shorter leaves, and inflorescences which reached maturity sooner than those of its sister plants. In Figure 21*C* the two central plants are different from the adjacent sister plants of the progeny and also differ as between themselves. The one on the left has coarse, upright, broad leaves, while the



VARIANT PLANTS MAY BE GIANTS OR DWARVES

Figure 20

In some instances the variant plants are taller and more vigorous than the other plants in a progeny, as shown in *A*. In other instances (*B-K*) the variants are smaller. The dwarfed plants shown in *C* and *D* produced no inflorescences. The other forms showed characteristic growth habits which differed distinctly from the other uniform plants in the progeny.

one on the right has finer leaves and lower flowering stalks which matured earlier.

The 31 progenies from Wisconsin (numbered 1 to 31, inclusive, in Table I) have a mean percent of variant types of 1.65, while the 14 progenies in which variant types appear have a mean percent of 3.65. The *t* test applied to the two means respectively (t=1.65/.401=4.11; $t_{.01}$=2.75; and t=3.65/.510=7.157; $t_{.01}$=3.012) indicates that the occurrence of the observed variants is not due to chance alone. Because of

DIFFERENCES IN FLOWERING HABITS

Figure 21

A shows a partially dwarf variant, with shorter leaves and with inflorescences which matured earlier than those of its sister plants. *B*—The plant in the center differs from its sister plants only in having a lighter green color and longer inflorescences. The two central plants shown in *C* both differ from the adjacent sister plants. That on the left has coarse upright broad leaves while the other variant has finer leaves and flowering stalks which matured earlier.

the small population in each progeny the increment in percent for each additional variant is very high. If a larger population in each progeny were available, two or several categories might be suggested.*

The population of 55 progenies numbered 35 to 89, inclusive (Table I), is sufficiently large to permit of a more detailed analysis. Two of the progenies (35 and 88) have variant types in a proportion much larger than the others, and apparently belong in a different category. These two progenies will be discussed later.

The frequencies of the 37 progenies which have a mean of 1.61 variant types are plotted for class intervals of .5 per cent (Figure 22). A Pearson curve of the type $Y=(y/a^{p})\ \ (a+x)p_{c}-px/a$ was fitted to these data, and the χ^2 test for goodness of fit yields a value of 14.485 ($\chi^2.05=15.508$). Hence the distribution appears to be of this type. From the analysis and from the plotted curve we may infer that the variant plants observed were not due to chance alone, and also that we may expect more of the progenies showing variants to fall in the interval between .5 per cent and two per cent variants than in any other interval of equal size.

The two progenies (35 and 88) (Figure 19, *D*, *E*) have 21.92 and 12.06 per cent respectively, of variant plants. The high frequency of variants suggests that many of the seeds were produced by some method other than parthenogenesis. The plants in each progeny are of the same general type since each possesses many common genetical characters, but many of the plants differ from the general type in other characters. If all or a large percentage of the seeds were formed by the union of male and female gametes, the appearance of characters common to all the plants suggests that the florets were self-pollinated and that there was a tendency towards homozygosity or they were produced in part by apomixis and the balance by self- or cross-pollination.

The morphological uniformity of sin-

*Statistical analysis by Paul R. Ozanne, Research Assistant, Wisconsin Agricultural Experiment Station.

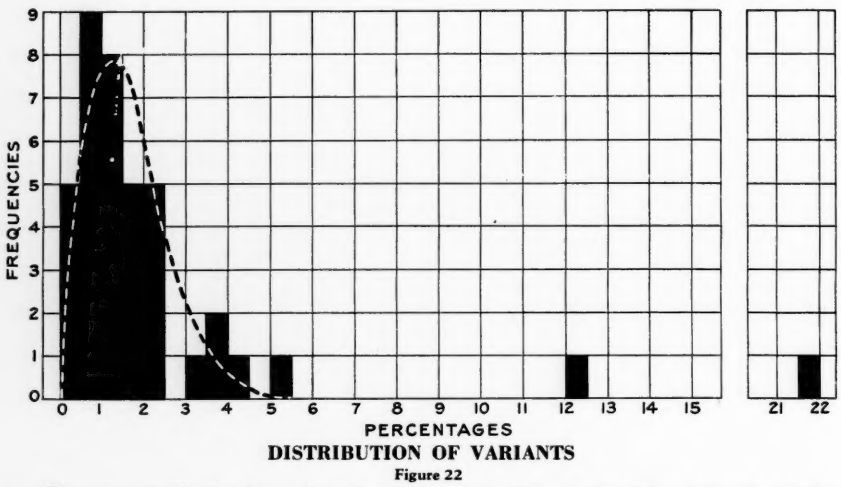


Figure 22

Frequency distribution for number of variant types in 37 progenies of *Poa pratensis*, and the calculated Pearson Type III curve.

gle plant progenies is due to diploid parthenogenesis,[13] and it is apparent from data shown in the foregoing discussion that this method of reproduction is the predominant type. From 102 single plant progenies representing a total population of 11,696 plants which originated in different States and countries, 48 of the progenies, or 33.07 per cent of the population showed no variant types (Table II). In the total population 1.59 per cent were considered to be variant types (Table I).

Discussion

Although the origin of the variant types is not known, possible methods can be suggested on the basis of a cytological study[13] of the origin of embryo sac and embryo. Haploid macrospores are produced but disintegrate in all observed instances; however, there is no apparent reason why an occasional macrospore should be prevented from functioning, and if an embryo sac were formed, the haploid egg might either produce an embryo by parthenogenesis or develop into an embryo following fertilization. Another possible origin of variant types is from the occasional fertilization of the diploid egg in an embryo sac which develops from a somatic cell of the nucellus. Pollen tubes are regularly observed in stylar tissue following pollination although tubes have not been observed in the embryo sac. Because of the infrequent occurrence of variant types it is almost impossible to determine their origin by cytological observations. The chromosome numbers of the variant types will be determined to compare with the number in sister plants, and this will provide evidence as to whether or not the variant plants are of hybrid origin. Since the meiotic divisions in both the microsporocytes and macrosporocytes are very irregular, gametes with various chromosome numbers may be formed. The irregularity of the chromosome behavior does not appear to affect the development of pollen grains, and if a gamete from such a pollen grain effects fertilization of either a haploid or a diploid egg, the hybrid would possess an aneuploid chromosome number approximately diploid or triploid.

It is evident from the data presented herein that variant plants appear, and it is suggested that these plants arise by gametic union or mutation. Once hybrid or mutant is produced, the type and the

newly combined chromosome complement either euploid or aneuploid may be perpetuated by diploid parthenogenesis. The occasional occurrence of progenies that appear to reproduce in part by sexual reproduction adds also to the formation of new morphological types.

The predominantly parthenogenetic origin of seeds in *Poa pratensis* can be used to advantage by the plant breeder and seedsman. The problem of isolation or maintenance of the purity of improved varieties is insignificant when seeds are produced by this process. The occasional variant plant will not contribute seriously to the impurity of a grass variety. The formation of new types increases the possibilities of selection in a species already rich with variants and potentialities for better adaptation to grazing under different environmental conditions.

The method adopted at the University of Wisconsin is to collect single inflorescences over wide areas from as many different habitats as possible. Seed from single plants from each collection are grown in rows. Each progeny is studied for disease resistance, drought resistance, leafiness, spreading ability, and other characteristics that are thought to be desirable for pasturage. Seed is harvested in bulk from the plants of the selected progenies, since in reality each progeny represents one genetically pure clone. The seed collected from the superior progenies is sown in blocks 10 by 20 feet, two replicates of which are grazed very closely, and two of which are grazed lightly. From this preliminary grazing test further selection is made, and these selections are tested more completely under different fertilizer and management treatments. Twenty-five plants from each inflorescence are sufficient to test for parthenogenetic seed-development and to provide seed for a preliminary grazing test. In a procedure such as this the breeding and testing program is expedited to the utmost. After single inflorescences are collected from pastures, seed from single clones is started in the greenhouse in late winter and transplanted to the field in the first summer, seed harvested in bulk and planted the second summer in replicated blocks for grazing, and observations and further selection of pure clones made under grazing conditions the third and succeeding summers. Collection of seed from a single clone achieves the same purpose as the laborious task of propagation by rhizomes, and at the same time labor is saved and sufficient material is obtained to use in a grazing experiment.

The plant row nursery is discarded after the second summer, during which the plants flower. The selected strains are maintained in a semi-permanent nursery which will permit obtaining pure seed of any of them in succeeding years.

The program for the improvement of *Poa pratensis* is limited to selection and production of clones by harvesting seed from single plant progenies. The great diversity of type already furnished by nature *may* provide more material than *might* be produced by *present known* methods of self-fertilization and hybridization (in *Poa pratensis*).

Table I - List of Poa pratensis L. progenies showing number of plants and percent of variant plants

Progeny number	Number of plants	Percent variant plants	Progeny number	Number of plants	Percent variant plants	Progeny number	Number of plants	Percent variant plants
1	51	0.00	35	166	21.92	69	237	1.27
2	103	2.91	36	130	2.00	70	81	1.19
3	99	1.69	37	194	2.55	71	204	3.95
4	60	3.33	38	128	1.56	72	22	.00
5	75	4.21	39	236	2.12	73	240	5.00
6	91	.00	40	73	1.37	74	236	4.24
7	75	.00	41	229	.44	75	67	1.49
8	53	1.96	42	218	.00	76	227	.44
9	48	2.08	43	218	.00	77	113	1.77
10	79	2.53	44	198	.51	78	231	3.03
11	49	8.16	45	138	1.45	79	109	.00
12	21	.00	46	161	1.86	80	200	0.92
13	29	6.90	47	96	.00	81	52	3.85
14	30	.00	48	174	.00	82	235	.43
15	98	3.45	49	218	2.31	83	263	.41
16	36	.00	50	224	1.79	84	240	1.67
17	33	.00	51	235	.85	85	222	1.35
18	77	2.70	52	218	2.29	86	239	.84
19	79	.00	53	225	.00	87	233	.00
20	77	.00	54	176	.00	88	141	12.06
21	20	5.00	55	224	.00	89	183	2.43
22	53	.00	56	127	.79	90	24	.00
23	25	.00	57	173	.57	91	26	.00
24	25	4.00	58	161	.62	92	26	.00
25	32	0.00	59	224	.00	93	26	7.69
26	46	2.17	60	108	.93	94	26	.00
27	24	.00	61	221	.00	95	26	.00
28	24	.00	62	234	.43	96	26	.00
29	22	.00	63	232	1.29	97	26	.00
30	41	.00	64	228	.00	98	26	.00
31	64	.00	65	0/	.00	99	26	.00
32	26	.00	66	243	.00	100	26	.00
33	26	.00	67	108	.00	101	26	.00
34	26	.00	68	106	.94	102	26	.00
						Total	12,506	1.79

Origin: 1-31 Wisconsin; 32 Virginia; 33-34 Missouri; 35-68 Ottawa; 69-89 Dunlop; 90-91 Norway; 92-97 Sweden; 98 Denmark; 99 England; 100 India; 101-102 U.S.S.R.

Table II.—Total Number of Plants and Progenies and Percent of Progenies according to Number of Variant Plants Per Progeny.

Class number of variant type plants	0	1	2	3	4	5	6	7	8	9	10	11	12	17	20	Total
Number of plants	2866	2067	1618	968	771	670	0	321	0	324	224	0	240	141	146	11,496
Number of progenies	46	21	14	9	5	5	0	1	0	1	1	0	1	1	1	102
Percent of progenies	45.07	20.59	13.73	8.82	4.90	4.90	0	0.98	0	0.98	0.98	0	0.98	0.98	0.98	100

Summary

No variant types were found in 48 out of 102 progenies grown as individual plants from seeds produced in single open-pollinated panicles of *Poa pratensis*. The highest percent of variants in any progeny was 21.9. In the total populatio 1.6 per cent were variant types.

The fact that a progeny is uniform throughout and practically identical with its seed parent indicates that the plants arose apomictically. The presence of atypical plants in an otherwise uniform progeny indicates that these plants may have arisen as a result of gametic union or mutation.

These breeding studies corroborate cytological findings that apomixis is the predominant type of reproduction.

Remarkable uniformity in type was observed among progenies collected in different climatic regions.

This predominantly parthenogenetic origin of seeds can be used to advantage by the plant breeder and seedsman. The problems of isolation and of maintaining purity in improved varieties are insignificant when seeds are produced by this type of reproduction.

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✦

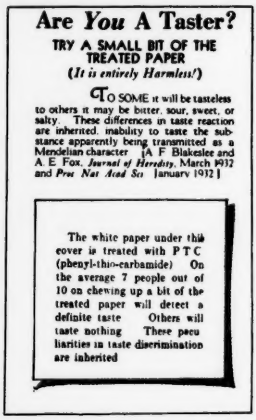
Pathological Heredity

THIS* is the fifth edition of the familiar and standard German compendium of human heredity, Baur-Fischer-Lenz. A number of changes have been made. This volume which deals with hereditary diseases and abnormalities has been doubled in size in comparison with the fourth edition. It appears as volume 1, part 2. The volume on normal traits (volume 1, part 1) is promised for 1941.

Lenz has continued as author of the chapters on eye, ear, skin, tumors and sterility, with little change from previous editions. Weitz has contributed the revisions of internal and nervous diseases, Verschuer the chapters on anomalies of body form, and infectious diseases, and Lange on mental diseases. The typography has been improved and a number of photographs have been added. A significant improvement has been the collection of all references at the end of each chapter, although for the most part they are incomplete and many of the inaccurate references have not been corrected. As in the past, English and American authors are under-represented in the text and bibliography.

Morton D. Schweitzer

*Lange, Johannes, Fritz Lenz, Otmar Frhr. von Verschuer, and Wilhelm Weitz. *Erbpathologie*. 526 pp. 213 illustrations. Lehmanns, Munich. 1940.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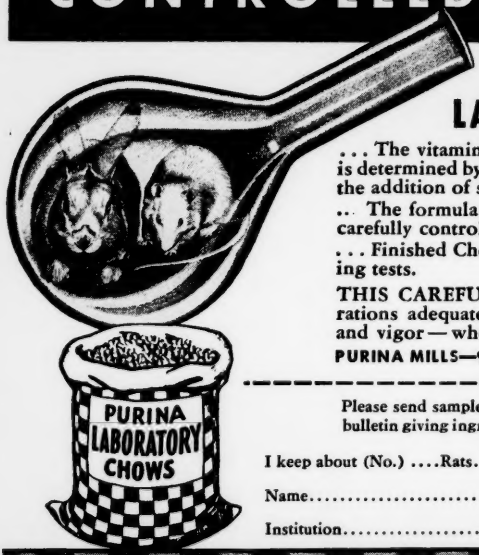

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